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REPORT BY:

THE GRILL FLAME  
SCIENTIFIC EVALUATION COMMITTEE



DECEMBER 1979

MR. MANFRED GALE, CHAIRMAN

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## SECURITY CLASSIFICATION GUIDE (U)

1. (S/NOFORN) All connection between official U.S. Government interest or participation in the generic field of Parapsychology is classified a minimum of SECRET, caveat NO FOREIGN DISSEMINATION. This connection includes the word parapsychology (or any derivative thereof) and U. S. Government. All data related to U.S. Government programs or interest in Parapsychology is disseminated on a strict, proven need-to-know basis only.
2. (U) The unclassified nickname for this subject is GRILL FLAME. Within DOD ACSI DA must approve in writing further dissemination or reproduction of this report. Future security planning for GRILL FLAME includes making it totally a special access program.
3. (U) Release of this report to cleared U.S. defense contractors and other U.S. Government agencies is contingent upon written approval of SECDEF or his designated representative. Release will be accomplished on a case-by-case basis.
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## Introduction (U)

1. (S/NOFORN) Background. In June 1979 it was suggested by Dr. LaBerge, then Under Secretary of the Army, that a Scientific Evaluation Committee be appointed to review the Army's parapsychological activities. In early July 1979, Dr. Ruth Davis recommended that because of the special interest of the Secretary of Defense, that the Committee review the total DOD posture and report directly to the GRILL FLAME Oversight Committee. ("GRILL FLAME" is the unclassified code word for any DOD or intelligence community association or involvement with parapsychological activities or interests; definitions of scientific areas discussed can be found in Chapter 5.)
2. (U) Mission and Organization. The Committee was organized by the Chairman (see Annex 1) and highly qualified members were invited to serve from various scientific disciplines. All members enjoy a reputation for an extremely high integrity and bring to the Committee a wealth of experience in experimental design and evaluation. The areas of expertise of Committee members include Psychiatry, Biostatistics, Psychology, Physics, Engineering, and Operations Research. Committee members were carefully screened to avoid any persons with preconceived notions for or against the subject under investigation, so that an objective assessment could be evolved. All Committee members represented themselves and were selected on their individual merits; therefore, views expressed are neither implicitly nor explicitly associated with their employing organizations. The listing of the organizational affiliation in the Annex is for identifying purposes only.

- a. (S/NOFORN) The mission of the Committee, known as the "GRILL FLAME Scientific Evaluation Committee" was as follows:

To review the parapsychological research, investigations, and applications within DOD and the intelligence community.

To assess the validity of claims made for the alleged existence of the PSI phenomena; with particular emphasis on the experiments which were instituted to approach the "proof of principle".

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To recommend a course of action for DOD in future parapsychological activities.

b. (S/NOFORN) The Committee visited all DOD installations involved in any aspect of parapsychological efforts and conducted additional visits and interviews with non-government sponsored investigators (see Annex 2). In addition, a large amount of classified reports, intelligence summaries, and open literature was reviewed. A collection of all such documents is stored in the Chairman's office and was made available to Committee members as required. In addition, available documentation applicable to the particular investigations in process or related materials was made available for inspection at all installations which were visited.

c. (S/NOFORN) Because of the DOD interest for eventual application and also because of the considerably greater activity, the bulk of the Committee's work was concerned with that portion of PSI research and applications known as "Remote Viewing" (RV). The work on Psychokinetics (PK) was also reviewed; however, since these investigations are concerned with the production of physical effects, there is considerably less controversy from the point of view of measurement techniques, but PK investigations share with RV the perplexing problems of understanding, controlling, and, indeed, proving the existence of a general phenomenon and the lack of ability to characterize the effect.

d. (S/NOFORN) Actually, the government-sponsored work in the area of parapsychology represents a very small portion of the total worldwide activity in this field.<sup>29</sup> Since 1972, the combined funding for DOD and the intelligence community was less than a total of \$1.5M.

3. (S/NOFORN) Report Overview. The following remarks pertain to the organization of the report and are intended to help the reader locate relevant information:

Chapters 1, 2, and 3 together constitute an Executive Summary of this report.

Chapter 2 - Major Findings

Chapter 3 - Recommendations

Chapter 4 is a chronological overview of parapsychological activities, providing baseline information input to the Committee. Any value judgments or critique contained in the overview are not attributable to the Committee's action, but are included in order to reflect as accurately as possible the recorded status just prior to the Committee's activities.

Chapter 5 defines the specific fields of para-psychology which are the subject of this report. This chapter also attempts to establish the intellectual gaps which exist in trying to relate various phenomena under the umbrella of para-psychology.

Chapter 6 is both a practical and tutorial approach to experimental evaluation, with emphasis on the role and limitations of statistical analysis vs. good experimental design and execution.

Chapter 7 summarizes the existing intelligence assessments of parapsychological activities in the Warsaw Pact countries.

Chapter 8 summarizes various theories proposed to describe paranormal functioning. The material is included primarily for the sake of completeness and also offers some editorial comment with respect to their collective merit.

Chapter 9 reviews and critiques the remote viewing work as carried out by SRI. This material is included in the main body of the report since the RV work at SRI is either directly or closely related to all RV experiments carried out by the DOD and the intelligence community under contract or in-house.

Annex 1 and 2 furnish detailed information on the Committee's members and their activities.

Annex 3 through 9 furnish background information and critical comments on many of the programs which were reviewed.

Annex 10 contains specific suggestions for the production of an improved protocol for any future research in RV.

Annex 11 - References

4. (U) General Observations.

a. S/NOFORN) All members of the Committee perceived a real need to carry out the assigned mission and approached this task with great diligence and utmost sincerity. The very diverse backgrounds and experiences of the Committee members assured that a wide spectrum of objective views was brought to bear on the subject. The prime motivation for the professional commitment invested by the Committee

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members was based on the high potential payoff which the parapsychological phenomena could have for the military and intelligence communities, if, indeed, such effects could be harnessed, controlled, and further advanced.

b. (U) The Committee in the course of its work gained a very great respect for the sincerity and dedication which the individual investigators brought to their respective tasks; in several cases, functioning under the handicap of a non-sympathetic management.

c. (C/NOFORN) On balance, the Committee has indeed been persuaded that there is some probability that effects attributed to the RV phenomena exist under unexplained circumstances and in conjunction with particular individuals. However, to date, the experimental techniques have not been adequate to document such effects.

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CHAPTER 2Major Findings  
& Observations (U)1. (U) Assessment of RV Phenomena.

a. (U) RV research and investigations thus far have not proved the existence of the phenomena and have not conclusively established any parametric dependencies. The same may be said about overall results based on current application-oriented activities.

b. (U) Many of the anecdotal events reported to this Committee as potential evidence of the existence of RV do not adequately sustain their claim under careful scrutiny. A few of the examples are subjectively spectacular, but lack of scientific procedures precludes their consideration as scientific evidence of the phenomena.

c. (U) On balance, the Committee has indeed been persuaded that there is some probability that effects attributed to the RV phenomena exist under unexplained circumstances and in conjunction with particular individuals. However, to date, the experimental techniques have not been adequate to document such effects.

d. (U) Even when granted the existence of the phenomena, careful attention to the consequences of false alarm rates in the achievement of useful performance levels would be paramount.

2. (C/NOFORN) Critique of Parapsychological Programs.

a. (C/NOFORN) Inadequate documentation and failure to apply adequate controls are the most frequent causes which limit the credibility that can be given to reports of "success" attributed to RV applications. In the judgment of the Committee, sole dependence on SRI-like protocols to resolve the RV issue will not be fruitful. Specifically, all RV programs reviewed included some form of subjective judgment of the degree of correlation; this factor and the ambiguous roles of the experimental designer, viewer, and interviewer are the two principal shortfalls.

b. (U) Operational programs, that by their very existence assume the reality of RV as given, may inadvertently establish the assumption in other communities that RV is real.

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c. (U) The possibility that present efforts can evaluate the existence of RV is seriously reduced because the work is carried out often by persons naive in the area of human experimentation at low budgetary support levels, with fragmentation of investigative efforts, using deficient experimental designs, and suffering from a lack of proper management direction. (Removing these deficiencies does not, however, guarantee that proof or quantification of the RV phenomena can be obtained.)

d. (S/NOFORN) Lack of proper management involvement, direction, and review was evident at all activities surveyed; and the government-sponsored RV program lacks focus, objectives, and top-down management review and control. This reflects in ambivalent direction and support at all agencies visited.

e. (S/NOFORN) Most DOD and government-sponsored work in the area of parapsychology has been application-oriented; in relation to the worldwide effort in this area of investigation, it represents a very small portion. There are currently more than 150<sup>21</sup> individuals, research institutes, universities, and professional societies in this country alone involved in parapsychological research and teaching activities. (Much of this work is also done under poor scientific procedures and in uncontrolled environments, especially as it concerns RV investigations.)

f. (U) The Committee found no evidence or any suggestion of fraudulent intent in any of the work examined.

### 3. (U) Parapsychological Research Standards.

a. (U) The conduct of parapsychological research to obtain scientific characterization and credible evidence of the parapsychological phenomena, would require an extremely disciplined and dedicated approach including:

(1) (U) Management commitment to a program which is sustained for an indefinite period of time at a cost of several million dollars per year.

(2) (U) Building essentially a new program, structured on an uncertain foundation, since very little data developed to date is suitable for further scientific extrapolation, except that previous research has established substantial knowledge of what not to do.

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(3) (U) Attracting a sufficient number of reputable and well qualified scientists from a variety of disciplines who are willing to dedicate substantial portions of their professional careers to this research.

(4) (U) Accommodation with substantial inhibitions in our society to this type of research, resulting in significant difficulties: (a) for conducting scientific investigations overtly; (b) recruiting and maintaining the high quality personnel required for this research; (c) publishing reports and exchanging data; and (d) establishing sufficient competition to obtain the required empirical replications.

(5) (U) Establishing test plans and procedures which are acceptable to the scientific community, which can be monitored by the sponsor for scientific and human-use integrity, and which are sufficiently rigorous to allow for experimental replication.

b. (U) Correct "statistical analyses" are a necessary, but not a sufficient condition for proper interpretation of data resulting from experiments of parapsychology. It is necessary to demonstrate more than statistical improbability; the quality of the data and the application of high scientific standards in the conduct and reporting of parapsychological experiments are at least as important as the statistical procedures used in evaluating the credibility of the results.

#### 4. (U) Psychokinetic (PK) Activities.

a. (S/NOFORN) The Army-sponsored experimentation at MICOM and the related contract with SRI, as a stand-alone effort to assess the potential effects on a computer-generated random bit stream, will not prove or disprove the existence of the PK phenomena.

b. (S/NOFORN) Research work in PK-related topics and detailed experimental planning has been carried out in several scientific institutions, including the investigations by Dr. Hawke at Livermore Laboratories, Dr. Jahn at Princeton University, and Dr. Phillips at Washington University. Committee members who visited Dr. Hawke's laboratory were very impressed by the scientific approach used in his investigations. His type of PK experiments (see also Annex 7) is seen as considerably more valuable than PK effects on random number generators, such as is planned at MICOM.

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5. (U) Status of Theoretical Knowledge.

a. (U) The Committee found that to date no adequate theory has been proposed to explain the mechanisms of the remote viewing process. Several basic mechanisms have, however, been suggested to explain psychoenergetic processes. To date, none of these theories is sufficiently persuasive from a scientific point of view or precisely congruent with empirical evidence to dictate the construction of a set of experimental designs that would lead to a verification of such a theory. (Most of the Committee believes that an understanding of parapsychological mechanisms is of secondary importance at this time.)

b. (U) There is no evidence of any unifying parapsychological concept or even a speculative notion which provides a basis for assuming that further understanding of any sub-category of PK or RV will help explain other phenomena associated with these parapsychological areas; for instance, obtaining statistically significant results in affecting the atomic collision process in a random generator device bears no known relationship to making remote viewing more reliable and repeatable. Positive results from unequivocal PK experiments would significantly increase the confidence of the scientific community to conduct other parapsychological experiments.

c. (U) The Committee was not exposed to any programs or suggested programs, which were adequately structured to prove or disprove the existence of the RV phenomena. Also, the Committee has not attempted to generate such a program; however, if a program were to emerge we would be very sympathetic towards recommending its implementation, since that would provide the justification for a serious scientific effort.

6. (C/NOFORN) Intelligence Considerations.

a. (S/NOFORN) Intelligence estimates of the quality and amount of parapsychological research activities in the Warsaw Pact countries are, admittedly, highly speculative, since insufficient and incomplete data are available for evaluation.

b. (S/NOFORN) Operational tests of RV are principally justified because of their potential high value in obtaining or supplementing intelligence information; however, the primary risk is that the test results may not be conclusive, either positively or negatively, with respect to the value of such techniques in an intelligence application.

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7. (C/NOFORN) Program Considerations. There are three potential major avenues of exploration available to DOD, which could be explored individually or in concert.

- a. (U) Proof of existence experiments.
- b. (U) Characterization of phenomena experiments.
- c. (C/NOFORN) Demonstration of utility through intelligence applications.

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## Recommendations (U)

1. (S/NOFORN) Proof of Existence. Work to demonstrate existence of the RV and PK phenomena should be supported if a credible approach were to emerge; however, it may be preferable to do this in some other agency other than DOD in order to more readily conduct the work in an open forum, which is necessary to subject the research to peer review.

2. (C/NOFORN) Characterization of Phenomenon. Parapsychological research (RV & PK) or related activities which have as their goal the scientific understanding and quantification of the phenomena, should not be sponsored until existence is established.

3. (S/NOFORN) Operational Applications. The Committee agreed that continuation of the operational endeavors does not necessarily imply that scientific proof has been demonstrated; however, the Committee was divided as to whether operational applications for intelligence programs can be carried out in an adequately controlled manner, sufficient to determine the usefulness or non-usefulness of the results. (See page 11a for minority opinion.)

SG1A (S/NOFORN) The majority (5 out of 8) believed that operationally-oriented RV activities aimed at determining the empirical value of RV to intelligence (like those at INSCOM [REDACTED] should continue, provided the following are done:

a. (C/NOFORN) Work must be monitored by an oversight committee that can review the work for its adequacy and guard against self-fulfilling prophecy. It should have members from the scientific and intelligence communities who can evaluate the adequacy of performance and reliability, as well as the requirements established by the user and provided to the operators. The false alarm rate should be considered in assessing the usefulness of the technique. Adequate review should occur periodically.

b. (C/NOFORN) Dependence on SRI approach should be phased out.

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c. (C/NOFORN) The attaining of useful data should not necessarily be attributed to the reality of RV phenomena.

d. (C/NOFORN) "Human-use" implications must be understood, properly authorized, and complied with, if applicable.

e. (C/NOFORN) Work should include adequate controls so that either value or non-value can be established.

4. (S/NOFORN) Current Programs. The RV work at AMSAA and the PK experiments at MICOM, along with the associated contractual supports from SRI, should be discontinued and terminated in the most cost-effective manner.

5. (C/NOFORN) Additional Future Activities.

a. (S/NOFORN) Although no significant military threat from parapsychological applications has been evidenced to date, the intelligence community should continue their collection efforts in this field in order to avoid any surprises.

b. (C/NOFORN) The progress of the parapsychological research being undertaken by the private sector in the U.S. and elsewhere in many laboratories and academic institutions<sup>29</sup> should be monitored and periodically reviewed via a DOD-assigned mission to an organization with competence in all relevant areas of science, with the view towards supporting or sponsoring such work as may be of interest to DOD.

6. (S/NOFORN) Management. A central DOD authority should be established to manage and fund the parapsychological program and monitoring activities. Management commitment to activities included in such a program should be unambiguous.

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## MINORITY OPINION (U)

(Onlansky, Holloway, Tang) (U)

(C/NOFORN) Operational Applications.

1. (C/NOFORN) Full evaluation of operational tests of "Remote Viewing" would require valid ground truth data, reliable scoring procedures, preliminary trials to establish adequate experimental procedures and whatever lead times are needed to conduct adequate tests wherever and whenever they may occur. Since operational tests can occur with little warning, it is difficult to assure that most of the conditions noted above can be satisfied in that type of program. Further, such tests cannot be varied systematically in order to provide a basis for evaluating the sensitivity of the results to operational procedures or variations among observers.
2. (C/NOFORN) A minority of the Committee believes that a test program in an operation-like environment is not likely to provide useful or reliable data. We see little to be gained by recommending operational tests.
3. (C/NOFORN) Such tests can also be dangerous. By encouraging the conduct of operational tests, this Committee endorses actions which have dubious scientific validity at best and can have dangerous implications for those who may rely on its products. At the very least, this Committee would be giving scientific credibility to operational activities exploiting phenomena that it elsewhere notes have not been proven or disproven.

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(U) **NOTE:** This chapter contains a chronological overview, providing baseline information input to the Committee. Any descriptions, value judgments or critique of reported results and investigations contained in this overview are not attributable to the Committee's actions, but are included in order to reflect as accurately as possible the recorded status just prior to the Committee's tasking.

**I. (U) General Investigations.**

a. (S/NOFORN) Stanford Research Institute. In 1971, two laser physicists, Dr. Hal Puthoff and Mr. Russell Targ became involved in a research program to scientifically investigate PSI. In 1972, they had the good fortune to acquire the talents of a psychic of some renown, Ingo Swann. Swann demonstrated the ability not only to remotely view targets at great distance from him, but also was able to somehow affect physical objects mentally. In a now famous experiment conducted in 1972, Swann was able to significantly disturb, on command, a superconducting magnetometer, which was as shielded as technology could make it. In 1973 a second formidable psychic came on board. He was Pat Price, a retired police inspector. Price had no trouble becoming extremely adept at RV. More recently, Hella Hammond has joined the SRI effort as a psychic; although was first picked as a control case and thought to have no ability whatsoever, she has performed some rather amazing feats.

**b. (U) CIA.**

(1) (S/NOFORN) In April 1972, the CIA became interested in potential intelligence applications of PSI. After discussions with Puthoff and Targ, a modest effort began.

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(2) (S/NOFORN) By October 1972, the investigation had expanded to allow a more complete research plan. The results were surprising, encouraging, and disputed within the CIA by skeptics from ORD\* and budding advocates within OSI\*.

(3) (S/NOFORN) In Summer 1973, Price, working only from a set of geographic coordinates read to him, provided striking descriptions of a "military-like" facility. As it turned out, the place was a sensitive NSA installation in West Virginia. Price was able to "penetrate" into the building and among other accurate descriptions, was able to correctly name people working there -- but most important, spoke out classified code-words he "read" from file folders.

(4) (S/NOFORN) Next, CIA decided to give Price the coordinates of a Soviet facility whose exact R&D mission was unknown. An extremely elaborate protocol was developed to guard against fraud or other criticism. Price again provided a great deal of information, but typically there was a mix of signal to noise; good data co-mingled with spurious. Of particular note, however, is a drawing and oral description of a large "crane" at the target site. Later, CIA analysts concluded that either Remote Viewing had taken place, or Price had intimate knowledge of the URDF-3 (an overhead platform).

(5) (S/NOFORN) It was decided to see if Price could operationally assist in technical collection efforts. In two tests checked against the interior plans of foreign embassies known to CIA audio specialists, Price correctly located the coderooms, leading the operations officer to agree that the method did have operational potential.

(6) (S/NOFORN) In Fall 1974, an experiment was conducted in-house by CIA engineers familiar with the SRI-Price coordinates protocol. The result was a description of what could only be a SA-5 missile training site at the coordinates. The Libyan desk officer was impressed, and indicated that a HUMINT agent had previously reported the same information.

(7) (S/NOFORN) In July 1975, Libyan coordinates were provided to Price, who came back with a description of a guerrilla training site -- again, confirmed by CIA analysts from clandestine agent reporting. The same month Price tragically died of a heart attack.

\*ORD: Office of Research and Development; OSI: Office of Scientific Intelligence

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(8) (S/NOFORN) Since July 1975, CIA interest has remained minimal and unofficial. The Agency claims to have stopped all work. The primary reason for abandoning the effort was fare of winning a "Golden Fleece Award".

c. (U) Army.

(1) (S/NOFORN) Army's interest in Psychoenergetic (PSI) goes back to 1972 and 1975 when the Surgeon General (MIIA), with DIA, published studies of Soviet/Bloc work.

(2) (S/NOFORN) In 1976, USAMICOM informally expressed interest in US replication of claimed Soviet experiments. SRI worked up a small program, and in August 1977, a one-year \$80K contract was let by MICOM. Work was to be accomplished under the innocuous title, "Investigation of Unconventional Discrimination Techniques." Of particular interest was Soviet efforts along the line of man-machine interface; e.g., radar operators continuing to guide AA missiles to the target despite the CRT images being obscured by chaff.

(3) (S/NOFORN) By Spring 1978, enough "demonstrations" of the phenomenon existed to warrant serious consideration for a comprehensive program to explore military application of PSI. Most promising were three subareas: Remote Viewing (RV), Psychokinesis (PK), and Telepathy. The Army ACSI was designated focal point to develop a DA program along the following criteria:

(a) moderate intensity, low profile; (b) balanced distribution of work -- avoid overlap and husband scarce resources; (c) balanced in-out house effort; (d) in-house effort first center on replicating SRI-type organizations' experiments; (e) consider both basic and applied research; and (f) fit within any future DOD program. A complete security envelope was placed over Army's interest in PSI, and the effort was given the unclassified nickname, GRILL FLAME.

(4) (S/NOFORN) In March 1978, Targ and Puthoff presented a talk on RV to a select AMSAA/DARCOM audience. This led to increased effort by AMSAA to explore RV in harmony with DA guidance, as it might apply to target acquisition and target description: (a) ascertain location and activity status of enemy units; (b) detect changes in status of places like enemy assembly areas; (c) detect, identify, and report activity of enemy equipment; and (d) provide real-time battle damage assessment. Also of prime interest was accessing enemy communications, and command and control systems.

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(5) (S/NOFORN) In July 1978, DIA sponsored a series of four unique experiments, in which intelligence analysts intimately familiar with target sites interfaced directly with the Remote Viewer. The DIA Project Officer had prepared three sets of Soviet/Bloc target coordinates, and one US control set. Even he did not know the coordinates, until the moment they were read aloud to the Viewer, Ingo Swann. Immediately upon hearing the coordinates, Swann verbalized about and drew sketches of the target. Only low correlation of target description to actual site was achieved in the first three trials. However, Swann provided a "moderate to high", lengthy description of the last target, a Soviet strategic missile field east of Moscow, a significant achievement. Most amazing perhaps, Swann passed through a silo cover, "traveling" down until reaching a connecting tunnel. Going through this passage way he entered a command and control room. One comment, among many, stands out: "The floors are paved in 'white tiles'."

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(6) (S/NOFORN) In August 1978, Dr. Vorona, DIA, chaired a meeting to develop a coordinated DOD program. A permanent working group, comprised of representatives from all interested agencies/services was established and has met a number of times to: (a) ensure scientifically acceptable experiments and evaluation methodologies; (b) ensure valid investigation of military applications of PSI and; (c) investigate significance of foreign technical and military PSI efforts. The working group also acts as a clearing house for information on the subject within the DOD framework, and monitors protocols for compliance with legal and medical guidelines.

(7) (S/NOFORN) In September-October 1978, Army ACSI and INSCOM designed a small program to narrowly examine purely intelligence application of RV. The concept was to familiarize (train) a cadre of personnel who had been subjectively culled from the whole Washington area INSCOM organization. Selection criteria included: above average intelligence, outgoing personality, adventuresome, open-minded, mature, artistic nature, and successful life career. It was preferred that the individual

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not have in-depth PSI knowledge or background. The following potential intelligence PSI applications were identified: locating and effecting undetected entry into denied enemy units facilities; SALT verification; advance warning of hostilities; "reading" enemy battle plans and intentions; tracking and locating key enemy clandestine HUMINT operations and activities. Implicit was the development of appropriate countermeasures to known or suspected enemy PSI capabilities.

(8) (S/NOFORN) AMSAA concluded a contract in September 1978 to have SRI conduct a number of experiments in support of the areas of interest outlined in paragraph 1c(4) above. These would be carried out in concert with the USACDEC, Fort Ord.

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(10) (S/NOFORN) Also on 13 February 1979, the GRILL FLAME DOD Steering Committee first met to hear and offer comments on the program's developmental status. Committee members included: all Service ACSI's, Dr. Ruth Davis, Dr. LaBerge, and Dr. Vorona (standing in for LTG Tighe). The tone of the meeting was a positive one.

(11) (U) Ms Volner, AGC, provided legal guidance on 15 February 1979, to the effect that any GRILL FLAME protocol should be reviewed by the Surgeon General's Human Use Testing Committee. If found to involve humans as subjects of research, or "risk" was entailed, appropriate HEW guidelines and AR 70-25 would apply.

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(12) (S/NOFORN) On 6 March SRI briefed, at his request, [REDACTED]

[REDACTED] Also on request, subsequent briefings were provided to him by DIA, CIA, and the Army.

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(13) (S/NOFORN) On 26 March SRI and AMSAA presented their RV-related protocols to the Surgeon General's ad hoc Human Use Subcommittee for GRILL FLAME. That panel judged the proposed RV work to be technology transfer and application oriented, rather than research or

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testing; therefore, HEW and AR 70-25 did not apply. It was recommended, however, that: (a) adequate scientific review of research protocols should be provided; (b) a behavioral scientist should participate in protocol execution; and (c) separate organizations like AMSAA and MICOM should develop Human Use Review Committees and processes. These recommendations will be incorporated whenever appropriate.

(14) (S/NOFORN) This week, MG Thompson requested the Surgeon General designate, by name, a fully qualified behavioral scientist to act as consultant during protocol development. In addition, the Deputy for Science and Technology in the Office of ASA(RDA), Dr. Yore, was directed by the Under Secretary of the Army to put together a small team of four-five highly regarded individuals of diverse backgrounds, to assist in protocol development, and to later monitor the work for its technical and scientific credibility.

2. (U) Other US Government "Players".

a. (S/NOFORN) USAF. Has maintained a small program over the past few years mainly to replicate known or claimed Soviet PSI experiments. Since March 1979, AF ACST has displayed a positive attitude toward the subject and it is possible that within a year the AF program will escalate significantly.

b. (S/NOFORN) Navy. In the early 70's, conducted some PSI experiments, but claims to have dropped out (perhaps because of criticism by press/Congress of "weird" projects). Suspicion lingers among Army project personnel that Navy may have nevertheless kept a tightly compartmented effort going. Navy representatives attend all DOD meetings, but remain uncommitted and literally silent.

3. (U) Current Status (1979) and Future Plans.

a. (S/NOFORN) MICOM. Concentrating on development of a program to exploit potential offered by PK. First, "Phase Zero" calls for replication and evaluation of certain experiments already designed and performed by out-house organizations like SRI. Funds are available for a complete program, but intention is to commit only some contractor support monies for Phase Zero; the remainder held in abeyance until that phase is completed. The following near-term actions expected within two weeks:

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(1) A committee of three senior scientists and engineers has been appointed to review the entire PK program; (2) the committee will select a qualified scientist as program director and; (3) the program director will be assured of a quality staff by the committee. Upon completion of Phase Zero the following objectives, to which fixed milestones have not been decided by MICOM, will be achieved: (1) develop a PK activated switch and; (2) conduct an intensive analytical effort. The PK-switch phase will involve development of specialized software, hardware and algorithyms. The analytical effort will include research of modern physics and formulation of a protocol to look for the PK "mechanism".

b. (S/NOFORN) INSCOM. Currently in the initial phase of familiarizing and introducing six RV specialists to the subject matter. Over the next six months they will discreetly filter out to SRI for "training" in RV techniques. The INSCOM contract is \$75K. Long term objectives include (1) refine specific abilities identified in each of the RV specialists; (2) establish intelligence collection procedures using RV and; (3) establish an institutional system for responding to validated tactical and strategic intelligence collection requirements (ICRs). Milestones:

|               |   |
|---------------|---|
| Feb 79-Jul 79 | Initial orientation                                   |
| May 79-Aug 79 | Individual training at SRI                            |
| Aug 79-Dec 80 | Refine specific individual abilities                  |
| Oct 79-Mar 80 | Establish response mechanism to ICR's                 |
| May 80-Dec 80 | Initial introduction of RV data to intelligence cycle |

Although only a very few INSCOM experiments have been conducted, all in the past two weeks, first cut analysis is encouraging with a moderate-to-high degree of target correlation. However, it is still too early to make any accurate assessments or predictions.

c. (S/NOFORN) AMSAA. On-going effort is divided between contractor (SRI) development of RV techniques; and AMSAA independent challenge/verification of those methods and outputs. Through March 1979, SRI accomplished the following of significance: (1) Remote Viewer(s) accurately located person(s) unknown to him, with four out of five first place blind judge matches and; (2) three experienced RV specialists scored well beyond chance in

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determining access codes to computers. Current contract with SRI is about \$190K. Between April 1979 and March 1980, SRI will perform as follows:

(S/NOFORN) TASK ONE:

- (1) Conduct RV sensing tests on Units/equipment at Fort Ord.
- (2) Provide to AMSAA relevant RV data, protocols, and procedures.
- (3) Provide guidance necessary to establish in-house RV program.
- (4) Conduct training of AMSAA personnel on a ten-choice numerical device.

(S/NOFORN) TASK TWO: Apply and evaluate RV technique relative to:

- (1) Tracking and locating key enemy personnel.
- (2) Detect change in status of military unit.
- (3) Rapidly determine damage resulting from non-nuclear attack.
- (4) Determine access code to computers and other electronic hardware.
- (5) Determine countermeasures to enemy RV.
- (6) Determine general context of enemy documents and other information items.

(S/NOFORN) Recently, a number of in-house RV trials were conducted, in accordance with established SRI protocols, with encouraging results. The first were of the "inbound and outbound" experimenter variety (beacon individual goes to target). The next phase (near term) will use the geographic coordinate protocol. Targets will be selected that are both distinctive in nature, and which can be correlated to tactical maps. Multiple viewers will be used in an attempt to better "fix" precise target locations. A major goal is to develop better procedures for technically demonstrating the existence or non-existence of RV. (NOTE: AMSAA and INSCOM work very closely on the PSI effort.

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Within the spirit of a true team effort, MICOM will probably fold in well with these organizations, resulting in the most efficient approach to the problem.)

4. (S/NOFORN) Funding. The funding for this program has been very modest since its inception. Except for some internal salaries which may have been paid from other sources, the total funding to date is approximately as shown below for the indicated calendar years:

- a. CIA, 1972-78: \$240K.
- b. DIA, 1979: \$100K.
- c. USAF, 1976-79: \$300K.
- d. Army, 1977-79: \$490K.

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**SECRET** **GRILL FLAME (U)**  
**CLOSE HOLD/HAND CARRY****CHAPTER 5****Parapsychology in Perspective (U)**

1. (S/NOFORN) The subject of parapsychological research and experiments, especially in the context of potential intelligence or military applications, consists of two dominant classes of psychic phenomena -- extrasensory perception (ESP) and psychokinesis (PK).

a. (U) Extrasensory perception includes topics such as telepathy, precognition, retrocognition, and clairvoyance. The primary subject of this report deals with a category of psychoenergetics generally under the umbrella of clairvoyance, but specifically known as Remote Viewing (RV).

b. (U) Remote viewing has been defined as an alleged ability of a person to sense information about a site, event, or person removed from any known sensory link. It has also been described as the acquisition of information not presented to any obvious sense; a perceptual process that acts as information input to the human subject.

c. (U) Psychokinesis (PK), on the other hand, does not refer to perception, but rather to a palpable disturbance of, or interaction with, another physical or biological system -- spontaneous or deliberate. Thus, it is the production of physical effects not mediated by any obvious mechanism. These are, therefore, perturbation processes that appear as an action output from a human subject.

2. (U) Under the label of "remote viewing", one generally assumes a spectrum of phenomena which are significantly different and to which some or all of the following attributes have been ascribed by the experimenters in the field: unlimited bandwidth; extremely high resolution; ability to cope with very high noise level; no attenuation due to medium traversed or distance traveled; independence of: a. target size and composition; b. observer to target ranges; c. temporal characteristics of events. Data which might confirm (or not confirm) the existence of one of these phenomenological characteristics would not necessarily confirm the existence of any or all of the others.

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3. (U) It is helpful to divide<sup>1</sup> the field of psychokinesis according to the magnitude of the energy transferred. For example, there are the so-called macroscopic PK effects, such as the spoon-bending exercises of Uri Geller<sup>2,3</sup>, the saltshaker levitations of the Russian woman, Kalagina<sup>4</sup>, and the self-levitations of the Frenchman, Girard. These have been very highly publicized, but to the best of our knowledge have evaded well controlled, systematic experimentation.

a. (U) Then there are PK experiments which involve much smaller amounts of energy transfer, where the effects are made evident by an inherently high gain in the experimental design itself. For example, magnetometers normally used for the detection of weak magnetic fields<sup>2</sup>, are very sensitive to slight displacement of their spools<sup>5</sup>, certain types of torsional pendula can transcribe infinitesimal forces into measurable deflection of a light beam<sup>6</sup>, electronic strain gauges routinely used for measuring propagation of elastic and plastic waves in solids can be used to detect very small disturbances of solid objects.

b. (C/NOFORN) Next, there is the so-called microscopic PK domain, where one is attempting to intervene at the atomic or nuclear scale of a physical system: to influence a radioactive decay process, for example, or the emission of an optical photon, or the atomic collision processes in a gas discharge. These are the sorts of processes involved in most of the random generator devices, one version of which is now in the process of design at SRI.

4. (U) In the course of reviewing and discussing research and related activities in the parapsychological field, one most often merges both the RV and PK areas into one topic without specific distinction as to phenomena, scientific curiosity, or potential application. The facts are that the functional definitions of RV and PK (as described above) are substantially different and that to date there exists no theory or even plausible concept which claims to associate one phenomena with the other; this also applies to the transfer of claimed skills in either areas. There exists no conceptual hint or scientific hunch as to how the knowledge or talent in RV, for instance, might be related to that of PK or how the various fields of PK are related to each other.

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5. (U) We suspect that the overriding criteria for interest in PK research (beyond curiosity) are due to the fact that physical systems are involved in the interactions, and we understand how to deal with them more precisely than when dealing with psychological phenomena. These are basically hands-off experiments, frequently involving a number of disciplines of several basic sciences to which we have grown accustomed in more conventional engineering tasks.

6. (U) In summary, the uniform treatment of all parapsychological activities, PK & RV, on the assumption that they could eventually fit into one scientific or technical discipline can serve only to detract from gaining further insight into the phenomena and dilute our understanding of the observed effects. Specifically, there is no evidence or conceptual notion which provides a basis for assuming that any further understanding of PK will help explain the RV process (or vice versa), or make remote viewing more reliable and repeatable.

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CHAPTER 6

Evaluation of Parapsychological Experiments (U)

1. (U) Introduction. Fisher<sup>7</sup> in his classic book on experimental design begins with a discussion of the grounds on which scientific evidence is disputed. In summary, critics who refuse to accept a scientific conclusion take one or both of the following lines of attack:

a. The design of the experiment is ill devised or badly executed.

b. The interpretation of the resulting experimental data is faulty.

If the scientific integrity of the investigators (proponents) were not at issue than a. and b. provide a useful framework for evaluating results and conclusions from any scientific experiment(s). However, in the case of parapsychological research, results are vulnerable to yet a third line of attack, namely fraud<sup>8, 9, 10, 11</sup>, i.e., the fudging of all or part of the presented data. Because of the frequent occurrence of documented fraud, it makes it very difficult for the disinterested observer to attempt to evaluate published data and conclusions concerning the existence of paranormal phenomena. The fact that any well planned deception would be difficult to detect (certainly from published descriptions) makes the task of evaluating even more difficult. It appears necessary (unfortunately) that the issue of investigator (those who are responsible for the planning) and experimenter (those who actually carry out the experiments) credibility must be addressed in attempting to evaluate research in this area.

2. (U) Guarding Against Deception. J. B. Rhine, a leading researcher in parapsychology, after discovering and exposing a case of data alteration (by a trusted colleague and friend) set out three guidelines for conducting and evaluating research in this area. These were summarized in Barber's excellent book<sup>12</sup> on pitfalls in human research, as follows:

a. "The necessity of trusting the experimenter's personal accuracy or honesty must be avoided as far as possible."

b. "A method that can help avoid reliance of the investigator's honesty is to involve a number of investigators in each study."

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c. "Each new experiment must be considered in effect only a pilot project until it is eventually repeated by others; and if an important finding is at stake, the more repetitions the better."

3. (S/NOFORN) Importance of Replication. We especially wish to emphasize the necessity of replication. In other scientific areas replication by independent laboratories is considered a vital step in the validating of any scientific conclusion. Barber points out that such cross-validation by independent workers is rare in parapsychology. In our opinion such independent replication is a scientific necessity if the existence of paranormal phenomena is to achieve any degree of credibility. Such replication could be either literal or constructive. Literal replication would include exact duplication of experimental procedures, measuring techniques, and methods of data analysis -- for example, following SRI's protocol for remote viewing (with or without same subjects). This is a natural and commonly used validation procedure. In fact, reported replications of the work of SRI on RV were essentially of this type. This was also to be the basis for MICOM's efforts in the area of PK (influencing the output of random physical process). The limitation of this type of replication is that even if similar positive results are obtained, one is still left with the possibility they were an artifact of the: a. design, b. the measurement methods, or c. method of analysis. This problem is somewhat reduced if the paradigm employed (design, measurement, and analysis) is widely accepted among the scientific community. This is certainly not the case for parapsychological experiments in general and is even more of a problem in the area of remote viewing. In fact, the problem of quantitating (measuring) the information in a target to be viewed and in the viewer description (transcript) is a major methodological problem. This along with other thorny issues (complicated scoring techniques) would not make the interpretation of results from such replications more (or less) credible. In constructive replication one begins with a clear statement of the empirical fact (ability to remote view). The investigator then is free to choose his design, measuring technique, and procedures for analysis. Positive results in this case provide stronger evidence for the empirical fact that is being advanced. Negative results, while they do not disprove the claim, certainly limit generalization (the phenomena exists in my laboratory, using my techniques, subjects, measurements, and data analysis).

4. (U) Relationship Between Experimental Design and Interpretation of Data.

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**UNCLASSIFIED****CLOSE HOLD/HAND CARRY****a. (U) Statistical Analysis.**

(1) Even if the possibility of fraud could be discounted (using Rhine's three suggestions would certainly help), published accounts claiming the existence of paranormal phenomena may well contain misleading results and conclusions. The major reasons being faulty experimental design (ill conceived or badly executed) and/or faulty interpretation of resulting data specifically when statistical methods are employed.

(2) It should be emphasized that the experimental design or plan provides the logical basis for any interpretation of resulting data. If the design is faulty or if carried out in a sloppy manner (by not strictly following protocol specifications) then any interpretation of results is faulty too. This point is essential to understand because of the widespread use of statistical methods, in particular, the statistical significance test, to support the claimed existence of paranormal phenomena.

b. (U) The Statistical Significance Test. In brief, a significance test is a statistical technique intended to assess, in probabilistic terms, how likely an experimental result is to have occurred "by chance alone". It is intended then to help rule out chance as a possible explanation for an experimental result. For example, a person who claims to be able to remote view participates in an experiment in which he is presented with six targets to view. A judge after visiting each target attempts to match each description (transcript) with one, and only one, of the six targets. This results in a perfect match, i.e., each transcript is successfully matched to the appropriate target. Such a result could have occurred by random pairing (guessing) of the six targets and transcripts. However, since the probability of this observed outcome is  $1/720 = .0014$  either a rare event has occurred by chance or some other explanation is more tenable. In this case the other explanation (offered by the proponent) is that remote viewing has been demonstrated. Deceptively simple, but what if the transcripts contained cues (counter explanation). The results of a significance test aimed at assessing the likelihood of some experimental outcome being due to chance does not provide proof that the explanation offered by the proponent is true. Too often the results of such significance tests are taken as proof for the advanced conclusions. Because of the widespread naive use of this procedure, one must be extremely cautious in relying on them in evaluating

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published results. The following are but a sample of the misuse of this technique:

(1) The naive interpretation that statistical significance implies proof.

(2) A single test of significance (single experiment) is meaningless. Only when an experiment can be conducted which when repeated gives similar results do we accumulate real evidence.

(3) Incorrect calculations (mistakes in arithmetic) or inappropriate methods.

(4) When they are applied to large amounts of data looking for interesting findings and when one or more are found they are published.

(5) Perhaps the most serious problem in interpreting positive results in the literature is that negative results are not usually published and hence unavailable. The literature is, therefore, biased in favor of positive results -- even though they may have occurred by chance.

5. (U) Summary. In a nutshell the quality of the data (and hence the interpretation) is more important than whether the "statistical analyses" are correct. This can only be insured by insisting that the conduct and reporting of parapsychological experiments meet the highest scientific standards. Any results from efforts which fall short of this goal should be, we believe, disbelieved. While honest differences of opinion may exist as to experimental approach or interpretation, the burden of proof lies with those who advocate the existence of any paranormal phenomena.

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CHAPTER 7

## Assessment of Warsaw Pact Parapsychological Activities (C/NOFORN)

1. (C/NOFORN) The scope of work in this area of research in the Soviet Union and Czechoslovakia is detailed in a document prepared by the U.S. Army Medical Intelligence and Information Agency.<sup>13</sup> In this document it is pointed out that beginning with early work (1930's) in the laboratory of L. Vasiliev (Leningrad Institute for Brain Research), Soviet efforts in the area of paranormal functioning have concentrated on behavior modification and control (e.g., putting people into a trance at a distance through hypnosis). This is in contrast to the Western orientation toward remote data acquisition. Also, apparently in keeping with their ideology, the Soviet's work is strongly oriented toward the physical aspects of the channel, such as determining the propagation mechanisms involved.

2. (C/NOFORN) The intelligence report<sup>13</sup> cited above points out that Soviet parapsychologists continue to face problems similar to those of their Western counterparts, in that observed phenomena are unstable and there is low probability of proving them in controlled tests under selected conditions. Soviet critics of the science have been quick to seize on these two characteristics in order to categorically reject many of the phenomena, and they have belittled some forms of such manifestations by contending that the conditions under which tests have been conducted have not been adequate to preclude fraud. In view of this situation, the Soviets will continue to investigate methodology, since they feel it absolutely necessary to quantify observed phenomena. Although they have not yet done so, the Soviets may very well be the first to identify the field forces involved and the means by which they are generated, due to their concentration on the mechanisms and energetics involved.

3. (U) A study by Garrett Airesearch,<sup>15, 16</sup> a review of the Soviet literature on psychoenergetic research, treats Soviet application of statistical theories, research done on electrostatics, the development of remote sensors, hypothesized carrier mechanisms, human sensitivity to magnetic fields, and training to improve psychoenergetic performance. Garrett concludes that the Soviet Bloc has had and probably still has an active interest and vigorous research program in this area.

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4. (U) The above report points to the increasing importance of the psychoenergetic area in Soviet research, an importance underlined in 1973 when the Soviet Psychological Association issued an unprecedented position paper calling on the Soviet Academy of Sciences to step up efforts in this area. The Association recommended that the newly-formed Psychological Institute within the Soviet Academy of Sciences and the Psychological Institute of the Academy of Pedagogical Sciences review the area and consider the creation of a new laboratory within one of the institutes to study persons with unusual abilities. It also recommended a comprehensive evaluation of experiments and theory by the Academy of Sciences' Institute of Biophysics and Institute for the Problems of Information Transmission.

5. (C/NOFORN) A most recently available intelligence report<sup>17</sup> confirms the continued interest of Warsaw Pact research in paranormal phenomena with strong emphasis on the basic mechanisms involved. However, it is concluded that most research is of questionable value and difficult to evaluate. The document also reports the following:

a. (C/NOFORN) Most of the current research on paranormal phenomena appears to be performed as an adjunct to other official duties; however, it is likely that some researchers are involved on a full-time basis and are receiving government support. The level of funding and extent of research is unknown at this time. At least three and possibly more officially-sponsored research groups exist in Moscow, Leningrad, and Alma-Ata.

b. (C/NOFORN) The striving for ideological acceptance has led researchers to create a variety of new terms for this research and to emphasize theoretical explanations based on known or yet-to-be discovered physical mechanisms. While this may lead to improved research in some areas, it could cause other possibilities to be ignored. An emphasis on application potential is also apparent. Ideological objections have usually given way, in the USSR, to practical considerations regardless of the controversial nature of a new idea or unusual phenomenon.

c. (C/NOFORN) Most Soviet research has been with people who demonstrate consistently high paranormal performance ability. It is known that there is a program to screen "gifted people" from the general population and that training techniques are used to enhance such abilities.

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d. (C/NOFORN) The data for assessing achievements in paranormal phenomena research are quite limited, and very little information is available from reliable researchers. Even their data usually lack sufficient backup material. This may be due, in some cases, to lack of proper publication channels for such research or possibly to factors of a political or security nature. Although limited, however, the data available from reliable researchers are highly significant.

e. (C/NOFORN) Very little quality research data on paranormal processes are available from Warsaw Pact countries. This is not necessarily an indication of a small research effort; lack of publishing opportunities, caution exercised by the researchers, and government controls (particularly for government-funded research) are likely reasons. Although results and conclusions are reported, the evaluation of experimental reliability is difficult, since sufficient procedural data are usually not provided, or sufficient experiments are not performed (or reported). For this reason, other factors such as status, credibility, and facility association of the researcher, have been considered in the evaluation.

f. (C/NOFORN) There are indications that the application of paranormal abilities in an intelligence mode is being considered in the USSR. However, significance of this interest or existence of specific intelligence-oriented research is unknown.

6. (C/NOFORN) The reasons for frequent iron-curtain representatives at Parapsychological Conferences may not be primarily stimulated by the opportunities to obtain technical information from U.S. experimental and research efforts, but may, in fact, be motivated in establishing contacts with selected attendees for intelligence purposes beyond the immediate subject matter under discussion at such conferences.

7. (C/NOFORN) In summary, neither the Soviets nor the Czechs are likely to abandon research on paranormal phenomena in the foreseeable future, although there will be more and more demand for objectivity in such research. Current research is concentrated on efforts to discover the basic mechanisms underlying phenomena of physical alteration of animate and inanimate objects at a distance by mental or other physiological energy transformations; such effects at present remain totally unexplained. Many Soviet and Czech parapsychologists are convinced that

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physical alteration is not different from psychical alteration, since both types of manifestations must eventually be proven to have an underlying scientific-mechanistic basis. It appears that their parapsychology research is now concentrated on the energetics of the problem and is aimed at achieving direct production and control of the energy involved.

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**UNCLASSIFIED** **CLOSE HOLD/HAND CARRY****CHAPTER 8****Review of Suggested Parapsychological Mechanisms (U)**

1. (U) To date, four basic physical mechanisms have been proposed to describe paranormal functioning on the basis of present theory or reasonable extensions of same. These are the ELF (extremely low frequency) electromagnetic hypothesis, the quantum correlation hypothesis, the extra-dimensional hypothesis, and the thermal noise theory.

2. (U) The ELF hypothesis suggests that psychoenergetic processes are carried by electromagnetic waves in the frequency region below 1 kHz.<sup>18-21</sup> Experimental support for this hypothesis is claimed on the basis of lower-than-inverse-square attenuation, low bit rates, and ineffectiveness of ordinary electromagnetic shielding; factors (among others) apparently common to both ELF and psychoenergetic processes. The quantum correlation hypothesis stems from the recognition that a theory of reality compatible with quantum theory cannot require spatially separated events to be independent,<sup>22-24</sup> but must permit interconnectedness of distant events in a manner that is contrary to ordinary experience.<sup>25-26</sup> The extradimensional hypothesis is based on the ideas of Targ, Puthoff, and May (SRI), G. Feinberg (Columbia University) and E. Rauscher (University of California Berkeley Laboratory) pertaining to the use of extra spatial and temporal dimensions to provide a space-time metric especially suitable for describing psychoenergetic processes.<sup>27</sup> It has been recently proposed by E. H. Walker that in psychokinesis (PK), mind influences a physical system not by transferring energy to it, but by utilizing the energy already present in the form of "thermal noise", i.e., the random fluctuations in the state of the system stemming from molecular heat motion. Mind is assumed to do this by identifying and selecting those low-probability states which give rise to the desired PK effect. This requires information, and one finds a relation between the information-processing rate of mind and the size of the PK effect.<sup>28</sup>

3. (U) None of these theories is sufficiently persuasive from a scientific point of view or reasonably congruent with empirical evidence in order to dedicate the structure of experimental designs to its verification. The current status of hypothetical underpinning for the possible existence of psychokinesis is more in the realm of recreation than theoretical foundation. Until substantial repeatability of psychoenergetic processes is established

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and until some basic parametric dependencies of the phenomenon are understood, it is not justifiable to pursue a course of action which pretends to verify any of the hypothesized mechanisms identified to date. Rather, it is expected that most likely a suitable theory will be evolved as basic data is developed from thoughtfully planned experiments, specifically directed toward achieving reliable replication.

DISCUSSION

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a. (U) Interest in paranormal psychology has ebbed and flowed in many countries and many laboratories since the early 1800s. Several professional societies, a few scientific periodicals, a couple of university departments, and a variety of popular publications have been devoted to the general subject matter.

b. (U) While numerous scientific investigations have been recorded in various types of paranormal functioning, the field has also been plagued with constant criticism from the "harder" sciences, including the more conventional experimental psychology. To cast more doubt upon the claims of paranormal functioning, there have been several reported and verified cases of fraud and falsification of data.

c. (U) As a result, skepticism of paranormal claims is generally maintained by most scientists outside the field. Paranormal researchers have thus been placed in a position of distrust, doubt, and often considered akin to magicians, charlatans, and writers of fiction. In essence, the paranormal researcher has been asked to prove his results and claims far beyond the levels of acceptance required by researchers in other scientific areas.

d. (U) Recently, considerable attention has been given to research publications emanating from Stanford Research Institute (SRI) in the areas of "remote viewing", a term used by Puthoff and Targ at SRI to describe their research in clairvoyant description of distant objects. Because these researchers are trained and recognized as "hard" scientists (i.e., physicists), they have achieved a much greater acceptance in some quarters than have the many researchers who have preceded them. Their publications in scientific journals such as "Nature" and "IEEE Proceedings" have augmented this reputation. Finally, they have, through their many publications, stimulated related and replicative studies.

e. (S/NOFORN) The SRI work, as well as that of others relating to their approach, has implications for covert intelligence information gathering. As a result, their research has been sponsored by several government organizations, both within and without the intelligence community.

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f. (U) Puthoff, Targ, and their associates have not gone unchallenged, however. Reputable scientists have evaluated and often criticized their methods, analyses, claims, and results. They have responded to such criticisms, publicly and apparently meaningfully.

g. (U) Thus, there exists a growing body of such "remote viewing" literature which has some very startling (to the non-believer) results, but which appears to be well planned and executed. Because the skeptics of these results are also vocal, quantitative, and respected in scientific quarters, the time has been taken by many to evaluate portions of this literature and to offer criticisms as well as support of it. The following comments are broken down by classification of experiment. Time and the extensive amount of experimental detail to support these comments prevent any more detailed description within the context of this report. However, more detail and supporting documentation can be made available.

2. (U) Evaluation of Experimental Types.

a. (S/NOFORN) Project SCANATE.

(1) (S/NOFORN) This series of studies relates to long distance remote viewing by specification of geographical coordinates. Targets which were viewed in this fashion include a West Virginia site, a Urals site, Kerguelen Island, Project Atlas, the Sylvania Laser Laboratory in California, the Berkeley Laboratory Bevatron in California, Utah and China Lake sites, and several USSR sites.

(2) (S/NOFORN) In general, the Kerguelen Island and West Virginia site results appear impressive. That is, the sketches and detailed verbal statements appear to match characteristics of the targets quite well until one carefully considers the timing and the notion that each subject could have obtained the impressive detailed information during the day that ensued between the first and second "readings" of each target. Specifically, following submission of the first reading, each subject chose to "visit" again and obtain additional more detailed information. This more detailed information is that which appears to provide a great match to the target characteristics. Similarly, inconsistent and conflicting detailed reporting causes the careful reader to be at least slightly suspicious. In all fairness, these were early attempts in the research program and the investigators were just beginning to feel their way. More critical evaluation should be applied to the longer distance and subsequent targets.

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(1) (U) The most extensive series of studies was undertaken with local targets, using individuals as "beacons". Several criticisms can be levied against this work, although, again viewed in total and with straightforward reading of the results, one is impressed with the degree of accuracy between some of the transcripts and the targets. Only upon much more careful analysis and critical comparison of multiple reports of the same studies, does one determine that there are inconsistencies in reporting as well as major experimental questions and likely flaws which can be raised. Major criticisms revolve around the selection of subjects and their original classification, as a first order of criticism. For example, the SRI investigators had originally planned to use three types of subjects (gifted, learners, and controls), and then subsequently decided to use only the gifted and the learners. Later, a third category (unselected volunteers) was added due to sponsor criticism and pressure. Upon careful examination, subjects who were originally considered learners somehow became gifted. There is some indication that subjects were categorized simultaneously in different groupings, and that in many cases, subjects had more impact upon the planning of the experiments than did the investigators. Lastly, various subjects also participated subsequently as experimenters, outbound experimenters, judges, and in other key portions of the research.

(2) (U) Another major criticism is the selection and preparation of the target pool. There is inconsistent reporting as to the size of the target pool, the person who selects the target pool, the means by which targets are selected from the pool, the individuality of targets in the pool, the specific naming of individual targets, and the number of persons related to the experimentation who are familiar with the target pool as well as individual target samples for a given experiment.

(3) (U) Other questions of concern to the experimental reviewer include the amount of pre-experimental orientation given to the subject, including the opportunity to learn various cueing techniques from the inbound experimenter; the actual selection of and behavior of the outbound experimenter; and the nature of feedback to the subject by the experimental team upon visiting the target subsequently.

(4) (U) Of the most major concern, however, is the nature of the judging procedure and criteria for

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for defining adequate responses. Specifically, one questions the order in which transcripts are evaluated by various judges, the selection of the judges (one judge is also a coauthor), the reason why the selected judge for many experiments happens to be the "best" judge used in an earlier multi-judge series, and the specific definition of the "target" which is used for judging. That is, the definition of the "target" in some cases appears to have been sufficiently vague that many responses could be determined to describe elements in the target complex, although the specific target is defined subsequently by the experimenters and after the judge has made his evaluation.

c. (U) Technology Target Studies. Within the SRI confines, a series of experiments was conducted to determine the degree to which subjects could identify specific technological elements used as targets. The target list included typewriters, machine shops, and the like. The results again are similar to those of other local target studies, and the reporting is equally inaccurate. For example, in one report all targets but one are the same as in another report. In the first, one target is an airplane simulator, while the same target in another report is a complete machine shop. These have little similarity, yet the reported data and judging responses are precisely the same. Clearly, this could be a typographical error, but it is representative of other reporting inaccuracies which run across the multiple experiments.

d. (U) Long-Distance Targets with Cueing.

(1) (U) Several experiments were conducted in which an individual known to the subject visited various unknown targets at a prearranged time. This type of experiment has also been replicated by other organizations, such as the group at Mundelein College in Chicago. The results are again typically spectacular. For example, the first such target visited, an airport in Costa Rica, is elegantly drawn and described. Later, we learn that this particular response was by Russell Targ, one of the experimenters who substituted his time for that of a missing subject on that day. Yet, Targ never again serves as a subject in any of the experiments, although his handwritten notations on that particular response are quite similar to handwritten notations of other subjects in subsequent experiments. Individual notations on drawings in this series appear to change somewhat from one publication to the next, ultimately causing concern on the part of the reviewer.

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(2) (U) There are similar detailed inaccuracies in the reporting of experiments done on the DARPA computer network, and with experiments conducted from one portion of the U.S. to another. A transcript of the Washington Square (New York City) fountain appears impressive at first, until one goes through a detailed analysis of the elements in this target and realizes that these elements, as described, closely approximate a variety of other urban environment targets. A closer approximation of these elements is, for instance, to Yankee Stadium, for the element matches to Yankee Stadium turn out to be slightly better than those to the actual target, Washington Square fountain. It is suspected that one could in fact find a target that would more closely approximate the transcript than would even Yankee Stadium, although no effort was made to do so.

3. (U) Summary Observations of SRI Studies to Date.

a. (U) It is recognized that many details, interpretations, and comments regarding the SRI experiments would be lengthy and perhaps difficult to comprehend, certainly impossible to present in a short report such as this. However, several concluding statements appear warranted and justified. These are as follows.

b. (U) Positive Characteristics.

(1) (U) A protocol has been developed within which apparently useful remote viewing of local and long-distance targets has been demonstrated. (The weaknesses of this protocol were discussed in the above evaluation.)

(2) (U) Some of the results, particularly the long-distance results, are difficult to explain except by the presence of a paranormal, remote viewing ability, or by use of uncontrolled experimental procedures.

(3) (U) The demonstrated remote viewing ability is alleged to be insensitive to time and distance.

(4) (U) Real-time, movement-containing activities can apparently be seen through this ability.

(5) (U) It has been claimed that untrained subjects can apparently demonstrate this ability and improve with practice, often providing information as valid as that of known "sensitives".

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(6) (U) The remote viewing channel is apparently quite noisy. Concepts of information theory pertinent to S/N improvement may some day be shown to apply to this channel as well.

c. (U) Negative Characteristics.

(1) (U) Research reports are of behavioral data, yet are not presented with sufficient, rigorous experimental detail appropriate to behavioral science publications and acceptable to behavioral scientists.

(2) (U) Conflicting, inaccurate reporting of experimental "facts" detracts from the acceptability of the results.

(3) (U) Large methodological weaknesses in the local target and long-distance target procedures provide alternate (i.e., non-paranormal) possibilities of explanation.

4. (U) Critique of SRI Investigations.

a. (U) Puthoff and Targ have received numerous criticisms, and have responded to such criticisms in depth. They have also chosen to publish a list of typical criticisms and their responses. Attached to this report, as Annex 8, is a summary of these criticisms, the responses to these criticisms by Puthoff and Targ, and comments regarding the appropriateness of these responses.

b. (U) Also attached to this report, as Annex 10, is a list of recommended research improvements to the basic protocol, which might be considered in beginning to improve upon this protocol. There is no guarantee that these are the best modifications or improvements, or that they are the only areas in which improvement is necessary; rather, they are offered as a beginning point for future researchers to consider modifications.

5. (U) Comments on Statistical Procedures Used by SRI.

a. (U) As part of the above discussed evaluation of the remote viewing literature, we had an opportunity to evaluate thoroughly the statistical analysis techniques used by Puthoff and Targ for the free-response transcripts. That analysis is attached to this report as Annex 9, and is summarized here.

b. (U) Essentially, the Morris procedure is a valid statistical analysis, at least to the extent that we are capable of evaluating it. Its limitation is that it only

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uses a portion of the data, that portion which relates the actual transcript of a given target to the target itself, and disregards the overall pattern of other transcripts as they are compared to the targets in question. It appears reasonable that a competent statistician might develop a free-response analysis technique which uses all the data in this matrix, rather than only the diagonal data.

c. (U) There are some limitations to this technique, as indicated in Annex 10. These pertain to the sampling with or without replacement problem, as well as to the number of targets and correct matches. Thus, the limitations are real, and the violation of these limitations and the effect of the violation of these limitations upon the sampling distribution are not precisely known. Unfortunately, Puthoff and Targ choose sample sizes which border on the edge of infringement of these violations. However, since their results are extremely unlikely by-chance prediction, it is most likely that the violations of the statistical assumptions are not severe enough to discredit the results for statistical reasons alone.

d. (U) The recently recommended sequential analysis technique, to permit continuing analysis of multiple response data until an acceptance or rejection criterion is met, appears to be an appropriate one. This approach has been used in statistical quality control applications for numerous years, and appears to have no known statistical flaws. Its application to this particular type of research is reasonable as long as all other statistical assumptions of sampling, independence, and the like are met. A thorough analysis of the experimental protocol and its bearing upon the statistical assumptions is more important than is a detailed analysis of the statistical technique itself, as the statistics are well established and beyond need for criticism.

#### 6. (U) Overall Summary.

a. (U) Without question, Puthoff and Targ have achieved much attention and some startling results; while reputable scientists have questioned the validity of all of their results. Simultaneously, their methodologies are in need of much improvement and subject to meaningful criticism. The only way that their concepts will be accepted by the scientific community is for non-reproachable experimenters to conduct similar research, using improved methods, without any possible intrusion by persons (such as Puthoff and Targ) having a vested interest in the results. It is, therefore, recommended that any future research which may be funded in this area give great amounts of thought to protocol improvements, rather than to mere replication of the existing research.

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b. (U) Mere replication of the current protocol, without elimination of these methodological flaws, will provide no additional useful information or results. Of primary importance in revision of this methodology is the need to establish objective report accuracies, based upon target elements rather than upon subjective matching. That is, an objective evaluation of all responses is required, one which is not subject to individual interpretation by judges or experimenters.

c. (U) Further, while advice from the SRI investigators should be welcomed and heeded in the conduct of this research and in modifications of the protocols to be used, they should simultaneously be sufficiently excluded from the conduct of the research so as to eliminate any possibility of criticism due to contamination of the results by these investigators.

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| <u>DATE</u> | <u>ORGANIZATION/LOCATION</u>   | <u>INDIVIDUALS CONTACTED</u>   |
|-------------|--|--|
| 23 Jul      | Combat Developments Experimentation Command, Fort Ord, CA                    | Dr. Bryson/COL Moses, et. al.  |
| 24-25 Jul   | SRI International, Menlo Park, CA  | Mr. Targ/Dr. Puthoff/ Dr. Tart   |
| 26 Jul      | Missile R&D Command, Redstone Arsenal, AL                                    | Dr. Jenkins/Mr. Clinton, et. al.   |
| 27 Jul      | Pentagon   | Executive Session, plus initial Army Materiel Systems Analysis Activity (AMSAA) Briefing by Mr. Kramer |
| 1 Aug       | National Security Council Executive Office Building                          | [REDACTED] SG11  |
| 4 Sep       | Central Intelligence Agency Langley, VA                                      | [REDACTED] SG1J  |
| 6 Sep       | Princeton University & Bell Labs, Holmdel, NJ                                | Dean John & Dr. Lucky  |
| 11 Sep      | Defense Intelligence Agency  | Dr. Vorona, [REDACTED] et. al.   |
| 26 Sep      | Pentagon   | SG1J Executive Session, plus [REDACTED] and Dr. Vorona, DIA  |
| 27 Sep      | Intelligence & Security Command, Ft. Meade, MD                               | SG11 LTC Watt, et. al.   |
| 28 Sep      | Army Materiel Systems Analysis Activity (AMSAA), Aberdeen Proving Ground, MD | COL Deprospero/Ms. Taylor/Mr. Copes, et. al.   |
| 6 Nov       | Lawrence Livermore Laboratories, CA  | Dr. Hawke SG11   |
| 1&7 Nov     | Foreign Technology Division Wright Patterson AFB, OH                         | Dr. Cacioppo [REDACTED] et. al.  |

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| <u>DATE</u> | <u>ORGANIZATION/LOCATION</u> | <u>INDIVIDUALS CONTACTED</u>    |
|-------------|------------------------------|---------------------------------|
| 288<br>Nov  | [REDACTED]                   |                                 |
| 9 Nov       | Pentagon                     | Executive Session/<br>LTC Watts |
| 10 Dec      | Pentagon                     | Executive Session -<br>FINAL    |

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## Comments Pertaining to AMSAA Investigations (U)

1. (S/NOFORN) We commend the AMSAA staff for their effort to attempt to provide complete and accurate reporting, and for their candid way of discussing their plans and results with the Committee. AMSAA proposes to replicate SRI with some cautions. This can, at best, test the integrity of the SRI reports; the real issue is to use procedures which can more reliably control efforts introduced more or less accidentally by experimenters with no expertise in dealing with human subjects. The objectives of the AMSAA work are unclear. The five types of CDEC targets chosen impose pre-or-post cognitive and real-time requirements. One type of experiment would seem sufficient-- particularly in view of the opportunities for confusion in such a target-rich environment as that chosen.
2. (S/NOFORN) The overall approach of allowing viewers to participate in experimental design--and statements made such as, "Viewers have to be believers", are troublesome. The restriction to one interviewer--who is also part of the experimental team--is a mistake. The judging process apparently only allows positive correlations--there are no penalties scored for negative correlations. Both static and dynamic situations are to be viewed--the implications to judging are unclear. The use of the SRI approach detracts from the value of these experiments as an independent approach. COL Deprospero made good sense in his comments--particularly about the need to find an approach which is not "evaluation sensitive".
3. (S/NOFORN) AMSAA started out by replicating the SRI protocols. This was done as part of an AMSAA learning process. They have also made changes in the SRI protocols so that: a. the integrity of the work will be improved; b. the dependence on matching transcripts with targets will become less probabilistic. AMSAA has employed SRI viewers and training procedures.
4. (S/NOFORN) Examination of SRI findings by this Committee has not resulted in a finding that SRI has proven anything of military operational value. We find the scientific discipline of the SRI effort leaves something to be desired. Most important, we believe that "matching" transcripts to targets and then finding "goodness" will never be of "go/no-go" value to the military. "Go/no-go" based upon statistical procedures simply can't be depended on.

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5. (C/NOFORN) It follows that replication of the SRI procedures, however great the integrity, will not move us further along the path of making certain what to do or not to do about RV. The military cannot be asked to believe a transcript produced from an interview; matched on a gestalt best fit, and scored statistically.

6. (S/NOFORN) The SRI effort should be terminated for the convenience of the government. The current AMSAA effort should also be terminated. We do not recommend that AMSAA do anything in the RV area at this time. However, if they were to do so, it should be to develop evidence, if such might exist, that RV can be identified on a deterministic basis. There are potential RV experimental designs which do not depend on "matching" by others to find if there is a "hit". Further, "hits" can be scored by equipment that cannot be influenced by human intervention--however well intentioned.

7. (S/NOFORN) AMSAA stepped off into an experimental world where they had no professional qualifications. Their intentions, however, to respond to a request to keep the program alive demonstrates their very commendable mission-oriented attitude. They have changed the SRI protocol to improve the integrity and lessen the dependence on statistical voting based on unstructured interviews. For now, though, let us quit spending money on two serially-connected, not understood human processes and then wonder why statistical analysis of such processes leaves our minds filled with doubt.

8. (C/NOFORN) To repeat. If we cannot demonstrate RV-- deterministically--on simple targets, why should we believe that it exists for complex targets with such reliability that military worth exists?

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## Comments Pertaining to MICOM Investigations (U)

1. (S/NOFORN) Formally, the MICOM proposal purports to replicate a hands-off experiment proposed by workers at SRI. The goal of this experimental effort is to provide an unambiguous documentation that the PK phenomenon exists. At its simplest, a Haitz noise source, a  $^{147}\text{Pm}$  beta decay source, and a pseudorandom shift register would be used as a means of generating a series of "random" numbers. It will be confirmed that each of these sources will provide a signal of "random" numbers.
2. (S/NOFORN) The experiment would involve first the documentation that without a subject present the output is a random sequence of numbers and that with a subject present a non-random variation from this previously random sequences could be produced. An LSI-11 computer will be used to create the display, to assume some preselected sample rate, and analyze the data using sequential analysis as a method for identifying a "significant" result. (For details of numbers of trials, modifications of sampling signal sources and sampling rates, methods of producing subject feedback, and averaging procedures, see the MICOM proposal. For an outline of technical details concerning the proposed configuration of the system and the test procedures to insure the integrity of its components and the system per se, see SRI document dated August 1979 prepared by E. C. May, Ph.D.)
3. (S/NOFORN) After review of the proposed MICOM experimental protocol, we have no doubt that the scientists at MICOM will be able to assemble and insure the reliability of the physical aspects of the proposed systems. There remain a number of questions about the proposed work. The attractive feature of this work is the hands-off, non-subjective nature of the experimental procedures. As one begins to examine the details of this effort, although this strength continues to be apparent, a number of other aspects of the work tend to convince us that the implementation of this program is fraught with difficulties.
4. (S/NOFORN) The assumption that the proposed MICOM proposal is a "replication" of an SRI experiment does not appear to be well-founded. To date, the proposed experiment by SRI has not been performed and has yielded neither positive nor negative results, ergo, replication

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is impossible. Rather, the proposed work is an identical experiment to the one which will be carried out at SRI. Any technical difficulties that are the result of a design oversight should occur at both sites. The status of inferential strength that may be derived from an exact repetition and precise copying of an experiment at two sites is discussed by Dr. Tang in his comments. Since the only formal difference between the two proposed MICOM and SRI experiments will be the individuals who will carry out the experiments, we are faced with the remarkable fact that the effort seems to be designed as experimental control for the principal investigators per se.

5. (S/NOFORN) There is one sense in which the proposed experiment is a replication of previous work. The authors indicate that there have been 54 experiments of the sort proposed and that 35 have reported statistically significant results. All things being equal, one way to evaluate the proposed experiment is to assume that if a variation from randomness does occur, then one could say that of 55 experiments 36 have proved positive. If this experiment proves negative, then the conclusion would be that of 55 experiments 35 proved positive. If one accepts this way of examining the potential experimental results, then it is difficult to see how the proposed experiments might be decisive. A few other permutations of results can be stated but none improve the potential decisiveness of the experiments. It simply does not appear that given the state-of-the-art as represented in SRI/MICOM proposals that this set of parallel experiments will prove to be critical.

6. (S/NOFORN) On the conceptual level, the basis for assuming that there is any connection between RV and PK is not compelling. Yet, reportedly, the motive for creating the MICOM experiment was to obtain some method to establish the credibility of the RV phenomena. The investigators at SRI have observed that one of the "very talented" subjects was able to influence a shielded magnetometer. Beyond this, there appears to be only a vague argument that would necessarily connect PK and RV. For people who wish to accept that RV exists, the proposed PK experiment would be accepted as a convincing demonstration, but we doubt that the non-believer will be convinced. Direct demonstration that PK and RV phenomena exist would be necessary before even the friendly but prudent skeptic would accept their existence as proved.

7. (S/NOFORN) The assumption is apparently being made that negative results could logically be used to stop

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further experimentation in the RV and PK area. We are not convinced that this is the case. A negative result would not eliminate any of the so-called miracles that convince some that RV yields important results. Since many of the so-called clear proofs are doubted by skeptics because of the vagueness of the standards being utilized to confirm the match between the description and the target, this problem would remain no matter what the outcome of the MICOM experiment. (See review of RV experimentation.)

8. (S/NOFORN) If one assumes that one or more of the subjects in this experiment is present in the experimental situation when the output of the system judged to be non-random by sequential analytic procedures is used, then one will not be able to conclude that PK exists. Rather, one should be prepared to launch a rather large scale research program to investigate alternative explanations for the findings of the mechanisms, how they relate to subject characteristics, etc. Is the Army, or are other agencies within DOD, prepared to support such a program? Will such an effort split and fragment the capacity to investigate RV? Are the potential applications of RV the primary interest? If there is no intent or capacity to support an extensive and expensive follow-up research program, then it is very difficult to justify the support of the MICOM experiment. Such a commitment to further programs would probably be required whether the positive findings were reported by SRI or MICOM, presuming that positive findings could not be explained by gross error or improper data manipulation.

9. (U) It also appears that the investigators plan to average results across trials. Since each trial will be subjected to sequential analysis, this would not lead one to miss a "positive result" on a single trial. However, it may spuriously increase the N so that a very small deviation from "randomness" would be a reason to reject the null hypothesis. In any case to repeat the point from the foregoing paragraph in other terms, the rejection of the null hypothesis would not necessarily lead to interpretation that PK was responsible for the deviation from randomness.

10. (U) The Need and Timing for Replication. Since the proposed experiments have not yet demonstrated a set of results, the proposed work does not appear to be replication in the usual sense of that concept. If a replication as such is required, then it would be advisable to plan to carry out that work after the initial work had demonstrated some interesting results. In order for it to be a replication in the most useful sense, if the SRI

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group demonstrates some interesting findings, then the work should be replicated in principle by some totally independent group. Such a group would assume totally independent responsibility for its selection and calibration of its instrumentation and for the selection and management of the human subjects in the experiment. They might be drawn from the pool of individuals said to be talented in RV, or from other populations. Any group undertaking such replication would have available knowledge of the methods, analytic procedures used, and knowledge of both conceptual and practical problems encountered in the SRI experiments. All of these factors could be taken into account when designing the attempted replication.

11. (U) Instrumentation. In general, the equipment would seem to be adequate to the proposed task. It does have the disadvantage of limiting the range over which one might test for the influences of PK, since the physical phenomena that govern the generation of the "random" signal are sub-atomic events. There is a lack of clarity concerning whether the LSI-11 will be solely dedicated to the protocol presented.

12. (U) Experimental Procedures.

a. (S/NOFORN) The MICOM protocol does not appear to take adequate account of the role of the subjects and assumptions about subjects in the experimental design. In the protocol presented to this Committee, it was assumed that SRI could assure that the subjects have been selected appropriately and managed correctly from an experimental point of view while at the MICOM experimental facilities. An example of the problems this introduces may be seen in the choice of subjects from the SRI group (or in the present protocol from the AMSAA) that have "talent" in performing RV. This tends to support an implicit assumption that if any deviation with the subjects present occurs, then this finding should be taken as demonstration that RV is a credible phenomenon. No attempt to manipulate variables related to experience with remote viewing is noted.

b. (S/NOFORN) Another problem that follows from the human subjects aspect of the protocol is that it provides a ready explanation of positive results at SRI and negative results at MICOM. That is, it may be argued that the situation at MICOM was insensitive to some needs of the subject that must be satisfied before the subjects can display their talent for PK. In order to maintain the integrity of the proposed experiments, the experimental

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procedures should be fully independent in their management. This would include responsibility for the management of experimental subjects. In the most recent proposal by MICOM they assume responsibility for the selection and management of human subjects, but no person experienced in designing human research protocols is added to their scientific team. A person experienced in designing and carrying out psychological experiments in human engineering (e.g., concerning such factors as signal detection) should be on the MICOM scientific team.

c. (U) Perhaps the central problem with the research design is its tendency to equate rejection of the null hypothesis with proof of the positive assertion that PK exists. The problems of using statistical inference to reject randomness in a series which may be quite large is troubling. One might speculate that, given the claim that PK can modify the magnitude of physical forces, that an experiment might be designed that would use the modification of the physical aspect of a structure in a way that could unambiguously be measured. The resultant modification might provide a more convincing demonstration that an unexplained disturbance of physical relationships had occurred and that one potential explanation for the disturbance could be PK. (In principle, the acoustic emission experiment by Hawke might be an example of an attempt to do this.)

d. (U) The exact procedures that will be used to vary sampling rate from various random sources is unclear. It is possible that sampling rates will be faster than physiologic events in the brain.

13. (S/NOFORN) Relevance of this Experiment to Military Applications. According to the MICOM protocol, the military application of remote perturbation (RP, a synonym for PK) is that it "offers the potential for remote man/machine interactions with computers, locks, switches, codes and other sensitive or delicate mechanical or electronic apparatus, barred or held secure from ordinary physical contact or intervention." The relevance of an experiment on remotely influencing a random process to that of remotely influencing highly controlled processes such as computers, locks, switches and codes is neither suggested nor explained. Nor is it explained in what way processes found in computers are physically similar to those found in locks, switches and codes, so that positive findings on one may be regarded as relevant to the others. The same argument of reference applies also between the physical processes used in this experiment

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(beta decay, noise diode, pseudo random shift register) and those found in computers, locks, switches, and codes.

14. (S/NOFORN) Contribution of this Experiment. Since this experiment uses equipment and experimental procedures designed by SRI, it offers no unique contributions to the scientific literature that is not already available other than those of independent control over the construction of the apparatus and operation of apparatus by MICOM personnel. It may be that none of these factors is truly independent of previous experiments if construction of the equipment, subjects with previous expertise as remote viewers, and other items in the experiment are shared with SRI. Since MICOM and SRI experimenting may develop a close collaborative relationship, it may be that the MICOM workers may become less neutral with respect to the scientific issues at stake here. The significance of this speculation is that the entire experiment at MICOM should be monitored throughout by a truly neutral, third party, particularly since "independent" replication is the only discernible reason for this experiment.

15. (U) Experimental Design. Assuming adequate experimental control, the basic purpose is to detect whether non-random output from the apparatus can be associated with some influence exerted by subjects. Since the mechanism which may produce this effect is unknown, it is difficult to design an experiment which is truly relevant to the question. Statistically significant departures from randomness may simply mean that certain events occur rarely, not that they are caused by influences the experimenter believes he is bringing to bear. It is, to repeat, difficult to design an experiment without a presumed mechanism. Consideration should be given to modifying or adding to the independent variables specified for this requirement:

a. (U) Subjects. Subjects other than those judged as experienced remote viewers should be used to provide an independent pool of subjects.

b. (U) Feedback. This is presently conceived as a choice, still unspecified, of one or more video displays driven by the computer. To this might be added variants that are controlled by the experimenter without knowledge of the subject, i.e., be decoupled from the experiment proper and report to the subject pre-selected artificial success and failure at various levels.

16. (S/NOFORN) Instrument Calibration. There do not appear to be any real doubts about the technical capacity of MICOM investigators to properly calibrate the proposed

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experimental system and its elements. If one assumes that PK exists, then it should be noted that there is probably no way to control for its effects or the calibration procedures per se.

17. (U) Random Source Selection. The equipment complex to be assembled for the experiment contains three elements, the purpose or significance of which is nowhere propounded. One, the Haitz noise source, generates random noise pulses from a device whose output depends on atomic forces. (Electronic, subject to inter-atomic potentials.) The second, a B-decay source, generates a randomly timed output which depends on nuclear forces. The third is the complex of Boolean logic devices comprising the LSI-11 computer, whose outputs depend on the macroscopic movement of carrier electron motion in solid state components. Which of these elements does the PK subject influence? Why use two random sources (nuclear and atomic)? Is there to be an attempt to ascertain if PK operates on an atomic level but not on a nuclear force level? If the experimental result is positive for both random sources, is the inference to be drawn that PK operates on carrier electrons (or holes), or that it operates on both nuclear and atomic potentials? The lack of rationale for employment of two random noise sources colors the experiment with the notion that technical elaboration has received more attention than rigorous experimental design.

18. (S/NOFORN) Dilemma. From one point of view, it is difficult to see what can be learned from any replication of the remote perturbation experiments that have already been reported. Table 1 summarizes reports of 55 statistical significance levels that range from non-significant to  $2 \times 10^{-8}$ ; 18 (33 percent) are non-significant; the median value is in the order of  $10^{-2}$ . Any report(s) by MICOM must fall within this range and cannot change the fundamental thrust of the already existing data base that still must be explained. One replication that relates primarily to the credibility of 55 reports will not tell us anything new. What is really needed is a sufficiently different type of equipment that is able to test one or more hypotheses that may be formulated to explain these data. It is curious that MICOM offers no suggestions for hypotheses that might be tested. A minimal approach (which is not overly imaginative) could be to test the effectiveness of different types of shielding; that would, at least, explain the possibility of identifying certain explanatory mechanisms.

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19. (U) Summary.

a. (C/NOFORN) This experiment on random processes shows no obvious relevance to influencing highly controlled processes. If there is an interest in learning whether it is possible to influence computers, switches, and locks by remote means, it is suggested that the experiments use such devices. It will not demonstrate the existence or deny the existence of a PK effect.

b. (U) If this experiment shows that certain random processes appear to deviate statistically when humans try to perturb them, it will still be necessary to postulate the mechanism which is involved in order to understand what the experiment demonstrates. (For example, the effect of cosmic rays on computer "soft fails" has been scientifically documented.<sup>30</sup>)

c. (U) The choice of the particular random processes used in this experiment is not explained well except possibly for their convenience. In the absence of a mechanism, even positive effects (whatever that means) would require us to explore the possibility of perturbing other types of random processes in order to understand whether the observed effects are specific or general in nature.

d. (S/NOFORN) The most pressing motivation for work in this area appears to arise because of an interest in verifying the existence of RV (parapsychological effects). It is our opinion that the proposed PK experiments will not decisively contribute to the resolution of the questions about the existence or non-existence of RV. If questions concerning the credibility of the "demonstration of RV" are the most substantial concerns, then both the SRI and the MICOM PK experiments represent diversions. This line of argument leads to the recommendation that the PK effort as represented by the SRI/MICOM experiments be stopped and research effort concentrated on the resolution of questions about the "RV phenomena". Only if the U.S. Army or DOD were prepared to initiate a large scale research effort to follow up on either positive or negative results from the PK experiments should the current proposed SRI and MICOM work be continued.

e. (S/NOFORN) If the decision were made to pursue the line of research proposed in the PK proposal at SRI (we do not recommend it), we urge that no replication be initiated until the SRI results are in. This replication should be done totally independently of the SRI effort.

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Implicit in this recommendation is the recommendation that the agency or laboratory responsible for the replication assume full responsibility for the selection and management of human subjects. Given the high level of technical expertise in the physical sciences and computer technology of the MICOM personnel, they might have critical contributions to make to the construction and calibration of the experimental system; but given their overall interest in this area of research and their expertise in managing human subjects, it would be advisable to carry out the replication in a laboratory (either in-house or on contract) already experienced in carrying out human experimentation in human factors and performance.

f. (U) The importance of the review, critique, and replication of any finding concerning PK and RV by the general scientific community must be emphasized. None of the proposed areas of research concerning PK or RV will be truly credible until they have undergone the public examination by the scientific community.

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**CLOSE HOLD/HAND CARRY****ANNEX 5****Comments Pertaining to INSCOM Investigations (U)****1. (U) History.**

a. (S/NOFORN) Tasking. In the fall of 1978, the Intelligence and Security Command (INSCOM), Ft. Meade, MD was tasked to establish a program to examine potential use of psychoenergetics for intelligence purposes. Initially, specific interest has focused on that element now called Remote Viewing (RV). The INSCOM project (IGFP) has been and will be evolutionary in nature:

(1) Establish a training familiarization program utilizing specially selected INSCOM subjects (RVers).

(2) Establish RV intelligence collection techniques.

(3) Establish a system mechanism for responding to intelligence collection requirements (tasking by intelligence producers like DIA) so that RV-produced data is quickly and efficiently used.

**b. (S/NOFORN) Selection of RVers.**

(1) To accomplish the mission it was necessary to locate people who might possess requisite psychic talent. The approach here was to match a large body of candidates against a number of subjective traits observed by SRI over the years (an RVer profile).

(2) A total of 251 INSCOM personnel in the Baltimore/Washington area were considered. Out of the 251, 117 were interviewed in a "survey" which purported to determine attitudes about possible use of psychic phenomenon in intelligence collection.

(3) IGFP managers/interviewers were alert for individuals who were: well thought of by peers and supervisors, above average intelligence, self-confident, articulate, adventurous, open-minded, career successful, mature, and "emotionally stable". Additionally, artistic ability was desirable. Those who displayed unreasonable enthusiasm for or against psychoenergetics were eliminated from consideration. Also culled were those who, for personal or professional reasons, were uncomfortable with the concept of collecting foreign positive intelligence by psychoenergetics.

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(1) Of the 117 interviewed, 30-40 met the basic criteria outlined in paragraph 1b(3) above. However, it was impossible to reduce this number further based only on the RVer profile. Another round of factoring down was done by application of a criteria based on assignment availability (relative permanence in the area). The number of the candidates dropped to 12. The lesson here is that should an expansion of the IGFP be required, it will not be difficult to locate people who will do well in RV.

(2) More than 90% of all those interviewed considered psychic phenomenon to be real, and of practical value.

2. (U) Training.

a. (S/NOFORN) At the time the IGFP began, SRI was the only major serious organization exploring psychoenergetics. INSCOM was directed to conclude with SRI, a contract which called for a certain number of RV specialists to undergo SRI familiarization training.

b. (S/NOFORN) In February 1979, SRI researchers intimately familiar with the subject matter selected six of the final twelve candidates to train. This phase began in April and is expected to end in December 1979. In-house familiarization and training at Fort Meade started in February 1979 and is expected to continue indefinitely.

c. (S/NOFORN) INSCOM has generally followed the RV protocol first established by SRI, with an orientation toward collection of foreign positive intelligence. (Note: The Army Surgeon General's Human Use Review Panel for GRILL FLAME found this protocol to be "technology transfer" rather than R&D.) As of 1 Oct 79, more than 150 RV tests have been conducted at Fort Meade. Project personnel assess the results as moderately successful. Some of the RV cadre now routinely provide useful intelligence data with the RV technique. These individuals have progressed far beyond so-called "beacon" and basic geographic coordinates work, and are now engaged against real world intelligence targets--a kind of OJT.

d. (S/NOFORN) Lessons Learned During Training.

(1) There are a number of factors which appear to help successful RVers. First, they must sense a "seriousness of purpose" for the on-hand task.

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Frivolous coffee table tests seem to produce less usable data. The RVer must know and be motivated by the knowledge that his information is important, and that the reason for conducting a test goes beyond merely proving ("one more time") his ability to perform RV.

(2) The physical and social environment has a significant impact. The individual must feel that it is "OK" for him to perform this unique task; that he is not somehow considered an aberration or "kook". Physical surroundings must be comfortable, pleasing to the senses, and offer privacy, security, and quiet.

(3) If the RVer knows or believes that the viewing task is the most important event of his day, he is much more likely to develop good intelligence information. Outside influences (e.g., family problems, illness, job-related conflicts) have detrimental effects on his ability to do well. The bottom line is that the RVer must be totally committed to achieving positive results and allowed to achieve absolute mental concentration.

(4) It cannot, at this point, be said that familiarization training improves one's ability in RV. The collective data shows no training improvements, possibly because the whole organization is in a learning curve. Individually, however, there is noticeable improvement in specific RV abilities following such training.

(5) All persons involved in doing RV say they feel that they have learned much about the "process"; and are beginning to be able to distinguish between relevant (presumably psychoenergetic) target impressions and noise (fantasy, "analytical overlay" or whatever).

(6) Experience shows that a major problem is lack of suitable physical space. The project's present location is such that high levels of noise influence or abort RV sessions. Also, due to lack of appropriate office space, operational flexibility has been somewhat constrained.

(7) A serious problem surfaced during this phase: the availability of "Sunday hire" RVers to engage in sessions. The IGFP essentially operates on an ad hoc basis, with little or no promise of even minimal permanence. Existence or demise of the IGFP in fact rests on which way the winds that control scarce resources (time, funds, and people) blow. All RVers have other, normal duties in their parent organizations. As might be expected, this leads to

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considerable scheduling conflicts. Because IGFP enjoys what amounts to second priority, sessions are often cancelled or are impossible to schedule. It is fair to say that this issue has hindered progress and has been dysfunctional to the RV process itself. The latter point is that the RVer finds it difficult to muster requisite positive attitude and "seriousness of purpose", knowing that RV tasks rank below those of his primary duties.

3. (S/NOFORN) Operations. Introduction of the RV process into actual operations has been accomplished several times. This is not to say that the IGFP is ready for full operational employment. A great deal of further work is necessary to establish intelligence collection techniques. Also, no mechanical system for responding to tasking exists. Optimistically, some operational utility can be expected in 1981. First utilization of this special technique will most likely be along the lines of tip-off (or cueing) to other collection systems. These could then be brought to bear on the target of interest.

4. (U) Comments.

a. (S/NOFORN) The INSCOM activities are being guided by common sense and disciplined procedures. We should not lose sight of the fact that INSCOM is not engaged in a venture into science, but rather one of a utility nature. Personnel involved are professional intelligence officers representing the three major disciplines: human, photo, and signal intelligence.

b. (S/NOFORN) Notably, the IGFP gauges how good individual sessions are based on strict operational judgments (how much usable intelligence is produced). This is in stark contrast to projects in other places which rely on exotic, often flawed, statistical methodologies to evaluate the results.

c. (S/NOFORN) The body of wisdom being accumulated is not grounded on stagnant repetitions of the basic SRI RV drill first developed in the early 1970s. It is impressive that the project is moving into type tests in which several iterations (they call it "building an intelligence pyramid") on the same target seem to provide a more accurate, detailed picture of the site.

d. (S/NOFORN) Data on each session is impeccably maintained. In addition, the managers have developed several visual tools that lay out clearly the number of failures, successes and in-betweens. One is not forced to guess, or have to pry out, what has gone on at INSCOM

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5. (U) Suggested Improvements and Observations.

a. (S/NOFORN) The current level of personnel assets should be immediately stabilized for at least two years. Intelligence analysts should be a direct part of the effort. Because of disruptions caused by TDY trips, other job commitments, etc., personnel involved in GRILL FLAME should be fixed into some organization configuration controlled by the project officer. (Note: It is only fair to recognize that one cannot spend his whole day doing RV. Common sense should prevail and whenever possible project personnel should be released to work whenever needed.)

b. (U) An adequate work and administrative area is needed. It should meet the general criteria established in paragraphs 2d(2)(6) above.

c. (S/NOFORN) Because of the uniqueness and sensitivity of the project, extraordinary measures should be established, followed, and inspected, to ensure that those individuals involved in the project do not lose career standing in relation to peers. For example, a general officer review of all OERs and EERs appears to be warranted. There is no small danger that an individual's career, particularly under the present ad hoc personnel situation, might be irreparably damaged by rating officials who feel robbed of control of people under their supervision.

d. (S/NOFORN) Regardless of the apparent near-term potential offered by RV, the INSCOM's project status should remain one of familiarization and training. The work should not be prematurely thrust into the operational arena. For the next two years, INSCOM should be permitted the "luxury" of tightening up procedures and attempting product improvement (see Chapter 3, para 3). The project should have, should it be needed, support of any resources within DOD. An example might be USAF support in development and analyzing intelligence targets.

e. (U) INSCOM is to be commended for its logical, level-headed, and professional approach to a most curious problem.

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3. (U) Bell Telephone Laboratories.

a. (U) At Bell we spoke with Dr. Robert Lucky, the man who probably started the whole (serious) PSI debate. He did this during his tenure as editor of the IEEE Journal "Proceedings", by publishing the now famous Targ and Puthoff article, "A Perceptual Channel for Information Transfer Over Kilometer Distances; Historical Perspective and Recent Research."

b. (U) Bell has no on-going PSI research and given conservative management's feelings against the subject, never will.

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c. (U) Dr. Lucky believes that SRI does not have a bona fide scientific approach. He feels that good research is necessary, but should be carried out by a group of fully qualified scientists and engineers, with very tight experimental protocols.

d. (U) He considers the Targ & Puthoff IEEE piece a very important forward step, but underlines that to be credible the procedures they describe must be replicated by many other people and organizations.

e. (U) In 1975, Dr. Lucky attempted a series of six RV tests, following the SRI protocol. Using in-house Bell volunteers, the tests were for the most part failures. But, enough correlations of Subject transcript to actual target were present in one or two trials, that he calls the whole thing ambiguous. He concluded that the issue should be pursued further, but Bell's management called it quits.

f. (U) Lucky commented that he once proposed to Targ and Puthoff that they allow a "debugger" like Randy the Magician to watch an ARI RV session. They refused on the grounds that no one believes them anyhow, and did not feel that permitting an extremely vocal critic to intrude into their lives/work would accomplish anything. (Dr. Lucky conceded that they have a point, but still believes that critics' input has a place in the experimental design.)

g. (U) Dr. Lucky described the extremely poor treatment given by the news media to PSI research. In his mind it is roughly divided between distortion of the facts and outright lies. Since he was once chastised by his superiors, based on a fabricated story in one of the sensationalist journals, he no longer talks to reporters.

h. (U) He feels that legitimate, scientific research should be going on, but the best approach is to do it openly, perhaps with a sponsor like NSF. Peer review would be an important part of the process.

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Lawrence Livermore Laboratories1. Characteristics of Experiments.

a. Highly specific physical phenomena are recovered by instruments while PK "intent" is being "exerted" by a Subject. Thus a definite "result" is measured in terms of a physical phenomenon (e.g., grain fracture measured by physical measuring apparatus commonly used for such metallurgical experiments). Thus, for instance, grain fracture is looked for, its occurrence measured and control experiments performed.

b. Multiple measuring devices measuring different physical manifestations of the same physical phenomenon are made. (Although the AE experiments have not utilized this feature yet.)

c. Control experiments are performed.

d. No "mechanisms" are adduced. The experiments are phenomenological only. Cause "A" (the PK Subject's "will") is associated with specific physically describable and measurable result "B". The "mechanism" which relates "A" to "B" is not a subject of investigation. Thus "purity" of intent is present in the experiments.

e. The statistical probability of accidental occurrence of result "B" from natural sources other than cause "A" would appear very much lower than in Random Number Generator PK experiments.

2. General Comment.

a. Hawke's experiments are the most objective and scientific of the investigations we have reviewed. Physical measurements of specific phenomena are being (or will be) made. His type of PK experiments are, in my view, vastly more valuable than the PK effects on random number generators.

b. A means exists for conduct of these experiments on a compartmentalized basis: Since the experiments utilize measuring equipment commonly used in weapons development, "cover" and access restrictions are relatively straightforward. As an example, NWC China Lake is just now initiating development of contact fuzing utilizing (for the

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first time in the fuzing community) acoustic emission of materials transitioning the plastic deformation regime. A small development project on "AE applications to guided missile fuzing" would provide easily administered and controlled PK experiments by Hawke.

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## Evaluation of Potential SRI Criticisms

1. Background.

a. Because a great amount of attention has been drawn by the SRI work, Puthoff and Targ have also received their share of criticism from other sources. To combat this criticism, Puthoff provided us a brief summary entitled, "Potential Criticisms and Responses." It is well done, although our preceding evaluations tend to disagree with some of his "responses." Since it serves as a good summary of research philosophy, that paper is useful as a "straw man" for overall evaluation of the SRI methodology. Each potential criticism (C1 through C9) is repeated below, along with Puthoff's responses (R1 through R9) and our pertinent comments. Not all the criticism addressed in Chapter 9 is responded to in this Annex.

b. The use of this rebuttal method by Puthoff, however, does tend to draw attention away from other areas of potential criticism as it makes it appear as if these are the only methodological areas of potential criticism. Such is not the case. The potential criticisms and responses, on the other hand, are sufficiently important to warrant separate discussion here.

2. Criticisms.a. Experiment Selection.

C1: The experiments discussed could be selected out of a larger pool of experiments of which many are of poor quality.

R1: Selection of experiments for reporting does not take place; every experiment is entered as performed on a master log and is included in the statistical evaluations.

Comment: Reported experiments, sketches, and the like are clearly and understandably selected. There is no room in the journal or open literature reports for the 7000+ experiments run with Swann, nor for all experiments conducted with other subjects. Unfortunately, many of the other experiments are not reported, even in summary form, anywhere. Further, statistical analyses are not given for some experiments, and contain overlap for other series of experiments. This criticism appears valid.

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C2: Data for the reported experiments could be edited to show only the matching elements, the non-matching elements being discarded.

R2: Data associated with a given experiment remain unedited; all experiments are tape recorded and all data (tape transcripts, drawings, clay models) are included unedited in the data package to be judged and evaluated.

Comment: There is inconsistency, although sometimes minor, in the parallel publication of the same quoted transcripts. Presumably, judging cues are edited out. This has not been done consistently. This criticism is at least partially valid. It is also unclear as to how many viewings are allowed both prior to and following an experiment. From one publication, it appears that all transcripts are given to the judge. Although all data may be given to the judges, other readers and audiences are given selected data.

**c. Cueing.**

C3: This study could involve naivete in protocol that permits various forms of cueing, intentional or unintentional.

R3: The use of double-blind protocols ensures that none of the persons in contact with the Subject is aware of either the particular target or target pool; similarly, no one in contact with a judge is aware of the target-list/subject-output correspondence. For example, judges are not taken to target sites by knowledgeable persons, but rather proceed to the target sites, unaccompanied, on the basis of written instructions generated without knowledge of subject output.

Comment: It has been shown that the questions and comments offered by the experimenter could easily serve as perceived or subliminal (shaping) cues. Similarly, it has been shown that, in some experiments, the experimenter does know something about the target pool or has helped to select it. Other concerns about judging procedures and available information have already been raised.

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The authors use the term "double-blind" frequently. Yet, they are quite naive as to the behavioral science meaning of this term, a naivete which is apparent throughout their publications. Traditionally, "double-blind" refers to an experimenter who collects the data and who is "blind" to the purpose, theory, and potential nature of the results of the experiment. Similarly, the investigator is "blind" to the subjects, the data per se, and the data recording, reduction, and analysis. Thus, the experimenter has little influence on the results because he theoretically does not know what should be obtained, while the investigator is sufficiently blind to the direct subject contact and data so that he cannot influence the results. They are both partially "blind" in a sense, thus "double-blind". The word clearly does not apply in either the traditional, or in a meaningful, sense to the SRI protocol.

d. Educated Guess.

C4: A Subject may be able to guess as to which sites in a given area are likely to be chosen as targets, and may have familiarized himself with the locations.

R4: In the statistical judging procedure used, no advantage could be gained even if a Subject were to be given a list of possible target sites beforehand and encouraged to familiarize himself with the locations.

Even in such an extreme hypothetical case (no such procedure was ever used) where a Subject could not help but render a set of perfect descriptions of target sites, he still has the basic statistical problem of generating blind the correct target/description pair sequence upon which the statistical evaluation is based.

Comment: The response is quite correct for the statistical evaluation series. However, demonstration experiments, such as Grant's Tomb, Superdome, Washington Square, Ohio caves, West Virginia site, and all foreign sites are not subject to statistical evaluation. Previous comments have pointed out problems in the results for these targets. Thus, the criticism is at least partially valid.

e. Target Limitations.

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C5: If a Subject is given feedback after an experiment that today's target was a fountain, he knows that the following target is unlikely to be a fountain, since targets are chosen for unique differentiable qualities.

R5: The target pool in use (greater than 100 target sites) contains several fountains, several buildings, several parks, etc., and, therefore, the content of a given target, determined by random entry into the target pool, is essentially independent of the contents of other targets.

Comment: This criticism is at least partially valid. The target pool does not seem to be established prior to the beginning of all the experiments and Subjects did not have, say, two fountains, with the exception of two targets which appeared once for two Subjects. A sub-pool of targets was also selected from the larger pool; thus, this selection process may have eliminated the possibility of more than one type of target appearing.

f. Target Generality.

C6: Transcripts generated by Subjects are so general as to match anything. ("Sky is blue, grass is green.")

R6: Judging protocol involves differential matching. Therefore, true but general statements do not help a judge to preferentially assign a transcript to one site as opposed to another.

Comment: Again, this is a valid response for statistically judged targets, but not for many others (e.g., Washington Square versus Yankee Stadium). It should be clear that many transcripts, in fact, match many targets; i.e., the channel is noisy. Why then (and how) can so many excellent responses occur?

g. "Read-In" Matches.

C7: Given a transcript and a target, a judge can "read in" matches.

R7: Differential matching on a blind basis allows matches to be "read in" equally for non-corresponding as well as corresponding target/transcript pairs, and, therefore, provides no differential advantage.

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Comment: "Read-in" can occur for targets not judged statistically, as was often the case. Experimenter cues in the transcript can be helpful here. Such experimenter cueing, or the possibility thereof, must be eliminated by protocol revision.

**h. Inadequate Handling of Judging Materials.**

C8: Preparation of judging materials (transcript typing) may provide opportunity for a "leak", or perhaps degradation of typing ribbon may provide artifactual information as to order of experiments.

R8: Transcript typing is carried out in a random order by individuals kept blind to the key; one-time ribbons are used.

Comment: Typing cues are not nearly as important as transcript content and judge's prior knowledge. How does the tape get from the experimental room to the typist? The typed transcript from the typist to the judge? How are both stored? Other "security" problems seem more important than does the typewriter ribbon.

**i. Post Hoc Photography.**

C9: Photographs used to illustrate remote viewing results are taken after completion of the experiments, and, therefore, suffer from the fallacy of post hoc matching.

R9: All blind judging, matching, and statistical evaluation of the results (which is where the scientific issues are decided) are completed before photographs are taken; judges do not have access to photographs during their analysis, and, therefore, judges cannot be cued into correspondences observed post hoc.

Comment: Several temporal and content problems exist with photographs. Was the San Andres airfield photograph taken after the judging? Why do aspect angles of photographs always coincide with the direction from which the Subject "views" the target?

The authors have "in five years of self- and other criticism, . . . not found a way to fault either the experimental protocols or the conclusions derived therefrom." I do not agree, and I believe that careful evaluation amply documents numerous such faults. Annex 10 offers initial guidelines, within the general SRI approach, to improve this protocol and eliminate or reduce many of the criticisms.

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**UNCLASSIFIED****GRILL FLAME (U)  
CLOSE HOLD/HAND CARRY**ANNEX 9**Summary and Evaluation of Morris' (1972)  
Free-Response Analysis Technique**

1. Several techniques have been devised to permit evaluation of the correspondence between stimuli (i.e., targets) and responses (i.e., transcripts) to estimate the extent to which any given transcript is descriptive of any given target. Previous researchers have derived parametric statistical techniques for such, based upon the likelihood that a given number of matches of transcripts to targets would occur by chance. Others have developed rating or confidence scales to analyze such data. Most of these tests assume independence of matching (i.e., sampling with replacement), although Stuart (1942) devised a critical ratio test to handle those cases in which the judge's ratings or responses were not completely independent.

2. This independency problem is exemplified by Stuart's example of a tendency for a judge to avoid assigning any transcript a ranking of one for more than one target. If a judge has ranked Transcript A number one for Target A', Transcript B number one for Target B', Transcript C number one for Target C', and there are four targets and transcripts, then he is unlikely to rank anything other than Transcript D number one for Target D'.

3. While parametric tests have been devised to handle such dependencies, they are distribution based and have a small error in them, an error which becomes larger as N becomes small.

4. Morris (1972) offers a general formula for calculating the exact probability of a given sum (or less) of ranks for the preferential matching approach. He also provides a table for representative situation calculations.

a. Assume the procedure whereby there are four targets (A' through D') and four transcripts (A through D) which must be blindly matched, and that the four transcripts must be ranked one through four for each target. Then the correct ranks, summed across all four targets, can vary from 4 to 16. The data matrix is shown in Table C1. Following the procedural requirements, the sum in each target column is  $1 + 2 + 3 + 4 = 10$ . The diagonal (underlined) scores are the only ones used in the calculation of the summed ranks; thus, the sum of ranks in this example is 6. In general, if there are n targets (and n transcripts), the sum of ranks can vary from n to  $n^2$ , with an expected value under the null hypothesis of  $(1 + 2 + \dots + n)$ .

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**UNCLASSIFIED**TABLE C1. Example of Preferential Ranks Matrix.

| Transcripts | Targets  |          |          |          |
|-------------|----------|----------|----------|----------|
|             | A'       | B'       | C'       | D'       |
| A           | <u>2</u> | 3        | 3        | 2        |
| B           | 1        | <u>1</u> | 1        | 3        |
| C           | 3        | 4        | <u>2</u> | 4        |
| D           | 4        | 2        | 4        | <u>1</u> |

In general, let,

$s$  = the obtained sum of the diagonal (underlined) ranks,

$N$  = the number of transcripts,

$n$  = the number of targets, and

$l$  = zero and all positive integers not exceeding  $(s - n)/N$ .

b. The number of ways it is possible to obtain a given sum,  $s$ , is given by Uspensky (1937, p. 24) as:

$$\sum_{l=0}^{\left(\frac{s-n}{N}\right)} (-1)^l C_n^l C_{s-Nl-1}^{n-1}$$

where  $C_a^b$  is the number of combinations possible of  $b$  things taken  $a$  at a time. Restated in a more conventional combinatorial notation,

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$$\sum_{l=0}^N = (-1)^l \binom{n}{l} \binom{s-Nl-1}{n-1}$$

$$= (-1)^l \left( \frac{n!}{l!(n-l)!} \right) \left[ \frac{(s-Nl-1)!}{(n-1)!(s-Nl-n)!} \right] . \quad (C2)$$

c. We are concerned with the probability, under the null hypothesis, of occurrence of the obtained sum,  $s$ , or of any other smaller sum. Thus, we need to determine the probability of occurrence of all values from  $n$  to  $s$ . This summation is expressed by

$$\sum_{i=n}^s$$

d. The number of possible ways that the rankings in the data matrix can be assigned is  $N^n$ . Thus, the probability of occurrence of the given sum,  $s$ , or of any smaller sum is the summation of equation (C2) over values from  $n$  to  $s$ , divided by  $N^n$ . That is,

$$p(\leq s) = \frac{1}{N^n} \sum_{i=n}^s \sum_{l=0}^{\frac{s-n}{N}} (-1)^l \binom{n}{l} \binom{i-Nl-1}{n-1}$$

$$= \frac{1}{N^n} \sum_{i=n}^s \sum_{l=0}^{\frac{s-n}{N}} (-1)^l \left[ \frac{n!}{l!(n-l)!} \right] \left[ \frac{(i-Nl-1)!}{(n-1)!(i-Nl-n)!} \right] . \quad (C3)$$

For the example in Table C1, this equation is equal to:

$$\text{Prob. } (\leq 8) = \frac{1}{4^4} \sum_{l=0}^6 (-1)^l \left[ \frac{4!}{l!(4-l)!} \right] \left[ \frac{(i-4l-1)!}{3!(i-4l-4)!} \right]$$

$$= \frac{1}{256} (1 + 4 + 10)$$

$$= 0.059 . \quad 75$$

(C4)

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e. When values of  $l$  and  $s$  become large, the calculations become laborious, although not complex. Morris (1972) has calculated the critical values of  $s$  for one-tailed  $p$  values ranging from 0.20 to  $10^{-7}$ , assuming that  $N = n$ . If  $N \neq n$ , the above equation (C3) must be calculated, as it must for exact probability values or  $N > 12$ .

f. For the example given in Table C1, Morris's table gives a value of  $0.05 < p < 0.10$ , which agrees with the exact  $p$  value.

The method is statistically sound, although the Morris (1972) tabled values do not permit exact  $p$ -value determination. The diligent researcher would undoubtedly choose to perform the precise calculations by using equation (C3).

g. It must be noted that this statistical test is valid only if the rankings are assigned independently for each target. As Morris' points out (p. 406), the obtained  $p$ -values should be used only as a rough approximation in the case of one judge ranking a constant response transcript set to a constant target pool. This caution is emphasized especially in the case that (1)  $N$  is six or less, or (2) the judge has previously not assigned any transcript a rank of one on more than one occasion.

h. The first caution ( $N$  is less than 6) does not violate the sampling distribution of the statistics; rather, it suggests that a judge is more likely to be influenced by his memory of rank of transcripts applied to previous targets when the number of transcripts is small. When the number of transcripts is larger than 6, presumably the uncertainty

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increases to the extent that the judge's rankings approximate independent responses. No data are offered to support this notion.

i. The second caution is simply another means to assess the independence of the judge's rankings. If he has not redundantly ranked the same transcript one before, there is evidence he is not behaving independently, i.e., ranking with replacement. The caution seems reasonable.

j. Morris further indicates that either (1) or (2) is particularly pertinent if more than one-third of the number one rankings are correct and, therefore, contributing substantially to the small value of  $s$ . When a single judge and constant target pool are used, other statistical procedures should be devised and used, contrary to current practice among researchers.

##### 5. Improvement on the Method.

a. Two general techniques are validly offered by Morris (1972) to solve the nonindependence problem. In the first method, separate judges might be used for each "ranking of the targets". This wording would suggest that a single judge rank all targets against a given response transcript. Such a procedure would involve sequential visits to all targets and necessarily rely upon the judge's memory for at least some target details. A better method would be to have a different,

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single judge rank all transcripts for each target, thereby having no knowledge of the other targets in the pool or how the same set of transcripts might be ranked for any other target.

b. The second methodological improvement requires that a judge be given one response transcript and its target (unknown) plus "other similar non-target materials which are changed from one ranking to the next." That is, the judge might receive Transcript A along with materials describing Target A' and nontargets E', F', G', etc. (Table C1) If the number of targets (plus non-targets) is large, then  $n$  is greater than  $N$ , but equation (C3) can still be applied.

c. As  $N$  becomes large (that is, the number of targets in an  $N = n$  experiment becomes large), the judge's task becomes more difficult in the "standard" protocol; therefore, it may be more practical to increase  $n$  than  $N$ , and let each judge rank transcripts on only one target. A good rule of thumb, suggested by Morris (1972), might be to not use this exact test when  $nN$  is less than 35.

6. Summary.

a. While Morris (1972) published an important paper, and his analysis technique is followed by many researchers, there remains cause for concern. Certainly, it is more desirable to calculate the exact probability of a given s

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than to use the tabled value, and the calculation is not very complex or demanding.

b. Of greater importance is the problem of nonindependence of rankings by the same judge. Most researchers disregarded this problem, others argue it away by indicating that some judges do in fact rank the same transcript "one" on two or more targets. Neither is an acceptable approach; the second argument merely points out that independence existed (or a "mistake" was realized by the judge) on one specific set of responses. What is needed is a more thorough measure of exact probability which takes into account the degree of nonindependence, much as a covariant might be used in parametric analysis to remove confounded sources of variation.

c. Perhaps of the greatest heuristic concern in this method is its partial use of the data. For the case where  $n = N$ , only  $n$  of the  $n^2$  data points (ranks) are used. The  $(n^2 - n)$  unused data become large as  $N$  increases. For example, in the  $n = 4$  case, only 25% of the rankings enter into the analysis. In the  $n = 9$  case, only 11% of rankings are used! An exact probability method based on the correlational relationship in the total data matrix should be developed. It would potentially provide greater sensitivity and more confidence among readers unfamiliar with this particular area of research.

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ANNEX 10

## Recommended Research Improvements

1. General:

a. Lengthy, careful study is required to develop a safe, perhaps foolproof protocol. Experience with this general type of research will be required to refine the protocol further, to render it acceptable to the behavioral science research community. Such is beyond the scope of this report.

b. Rather, the following suggestions are offered for improvement in the experimental protocol used generally by Puthoff and Targ (and others). These improvements, when used in the context of a "local area" series of experiments, will yield valid results which can be used to address questions of channel capacity, phenomenon existence, learning rates, and the like. Thus, the suggested improvements are classified by experimental operation, much as is the published SRI protocol.

2. Target Pool Selection:

a. To carry out a series of  $n$  experiments, the target pool should be much greater than  $n$ . The target pool should be selected prior to the experiment and should contain distinctive targets. Once distinctive targets are chosen, however, there should be other similar targets selected, such as several fountains. These should have specific, individual details so that a general fountain description will not apply very well. Most important, the target pool should be selected by someone not involved with the experiment and unknown to the experimenters, investigators, Subjects, or judges. Further, the experimenters et al. should not know the size of the target pool.

b. Ideally, the targets and their locations should be totally unfamiliar to the experimenters, investigators, Subjects, and judges. For example, the targets could be selected in and the experiments conducted in a city totally unfamiliar to the above individuals. In this manner, cueing and reading-in are less likely. Each target should be listed on a separate card and should include what aspects of the target are to be viewed, e.g., the fountain in a plaza, and from what viewpoint. The particular distinguishing aspects should also be

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noted as well as unique, meaningful behavior of a target person for that specific target. The description should then be enclosed in an opaque envelope and sealed. The envelopes should then be thoroughly randomized. No numbering system is necessary. The targets should be stored in a safe or container inaccessible to the experimenters, investigators, Subjects, and judges. Further, the location of the safe or container should be unknown to the experimenters et al.

3. Investigator. This is the person or persons who designs the experiments and is familiar with the literature. He does not collect data, select targets, prepare transcripts, analyze data, or in any way interact with elements of the experiment in a manner by which he might deliberately or unintentionally affect the experiment or its outcome. In a word, he remains "hands off".

4. Subjects.

a. Subjects can be experienced or inexperienced, as the purposes of the experiment dictate. As long as the Subjects remain totally unininvolved in other aspects of the experiments, their characteristics are less important. They should not serve also as experimenters, judges, co-authors, and target beacons.

b. Further, they should not be close friends of the experimenters, investigators, or judges. With such a lack of personal familiarity, idiosyncratic behavior by the Subjects or investigators is less likely to serve as a useful cue to the judge.

c. It is assumed that an intelligence application of remote viewing would, necessarily and desirably, use the same Subject(s) repeatedly. Thus, successful Subjects should logically serve consistently in that capacity. However, while in a research mode, when the information channel is being quantified, care must be taken to avoid artifactual results due to data contamination from Subject/experimenter communication. The lack of repeated use of Targ as a Subject is thus supported, even though he provided an excellent response to the San Andres airport. (One must wonder why he wasn't used again in view of this highly accurate response!)

5. Experimenters.

a. Although we fail to see the need for an experimenter to be present during the actual transcription, if one is used, this person must be totally unfamiliar with the

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target pool, selection procedure, target beacon, and as many of the other details of the experiment as possible. A defined procedure should be established to make the Subject feel at ease, and assure him/her that remote viewing is acceptable. Although it would appear unnecessary to repeat this procedure with experienced Subjects, to keep this portion of the experiment standardized it would be best to repeat these instructions. No previous results should be shown. In addition, a Subject should not be told what kinds of elements or aspects are to be used in their description of the target, but rather to describe the perception of the target as accurately as possible.

b. If an experimenter is present during the actual transcription, a pre-set list of innocuous questions might be used. These should be used only if the Subject seems to be totally unable to continue describing any aspect of the target.

#### 6. Target Beacons.

a. If one or more target beacons are used, the number of these should be specified in advance and then remain constant. It is understandable that they must be known to the Subject. However, this does not mean that they must be present at the site from which the viewing takes place. Since the Subject does not appear to "track" the target beacon prior to the start of the experiment, every effort should be made to keep the Subject and target beacon at a maximal physical distance before, during, and after an experiment. This is easily accomplished if the targets are located at a physical distance, such as in another city.

b. The target beacon should receive the target designation and description from a person totally unconnected with the experiment and unfamiliar to the experimenter. This person would not know the contents of the target pool and would select, on a predefined random basis, one envelope from the target pool. This person would relay the target envelope to the target beacon at a predesignated location distant from the location of the target pool and the target.

c. An experiment would begin at a predesignated hour on predesignated days, the number of which would also be preset, for both individual Subjects, as well as for the total experiment.

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d. Targets should be used without replacement, essentially for the reasons stated by Puthoff and Targ.

e. The target beacon should proceed to the designated target and view the preselected elements of that target as specified on the target card. The target beacon should do only these prespecified activities, which should be uniquely meaningful interactions with that particular target.

7. Subject Responses. The Subject should begin his/her description of the target at the predesignated viewing time. No prior viewing should be allowed. The description should be tape-recorded and should include all experimenter questions if an experimenter is present, although, again, I see no need for such. (An uninterrupted videotape should also be used to verify the absence of nonverbal experimenter cueing.) A Subject should be allowed to sketch or model if he/she so desires, but this should also be predetermined by the Subject and held constant for each experiment. A Subject may be encouraged to be as specific as possible, but not told what kinds of elements to include. Only one viewing should be allowed.

8. Feedback.

a. Feedback and no-feedback experiments should be conducted. In a feedback situation, only the Subject (not the experimenter) should receive the feedback. The contents of the target envelope can be transmitted to the Subject. Neither the target beacon nor the experimenter with the Subject need have any knowledge of the Subject's response to the target or a description of the target.

b. The tape recordings and sketches or models should be dated, sealed, and immediately forwarded to an independent person totally unrelated to the experiment and unfamiliar with all persons thus far associated with the experiment. This individual should hold all data until the judging procedure is complete. This person will also type and edit the tapes, eliminating all references to previous targets, including any experimenter's question, should they not conform to the criteria for experimenter's questions. A target description should be included with the packet.

9. Judges and Judging.

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a. Effort should be placed on the development of objective judging criteria, perhaps measured by an item count or content analysis, semantic content, or other techniques better known to psycholinguists. Type/token ratio approaches might be modified to meet these needs. In any case, objective criteria for the judging procedure would greatly reduce the subjective element in this phase of the experiment.

b. Several judges should be chosen who are unfamiliar with the experiment and unknown to those who have participated thus far. While they may be selected on the basis of certain personal attributes (e.g., artistic ability, intelligence, sponsor representatives), they should have no professional interest in the research. That is, they should not be magicians, consultants to the project, co-authors, fellow researchers, etc.

c. Each judge should proceed to each target location, ordered randomly with the edited tape, associated drawings or models, and the target description card. No judge should be given a list of the targets. Each judge should proceed to the targets in a different random order. At each target, the judge should rank all transcripts against that target, as stated on the target card. The judge would return his rankings to the individual who held the transcripts earlier and would then be given the second target location, continuing until all transcripts have been ranked against all targets.

#### 10. Data Analysis.

a. Once the judges have completed the ranking procedure, all associated data should be turned over to another person thus far unrelated and unfamiliar with the experiment and other persons associated with the experiment.

b. Until an a priori judging criterion based upon target/response content is developed, the Morris (1972) statistical approach can be followed. Morris' small sample and replacement cautions must be heeded and satisfied.

c. To be safe, an experimental series should be large, on the order of greater than 15 targets per series. This requires a target pool on the order of 200 targets.

d. Since Morris' technique reveals statistics based on differential judging among targets, additional targets could be added to the judges' target list; i.e., they might actually visit more targets than were actually

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used and rank all transcripts against all targets, used and not used. This approach would serve to assure a ranking activity, rather than a best-case matching approach by the judges.

e. While research using larger numbers of targets and more persons is clearly expensive, the "cleaner" results warrant the additional cost. Greater data from a greater number of judges would result in further data stability. Since judging is subjective even under a better defined set of response criteria, in that there is always some degree of interpretation of what a subject says, the increased use of judges seems beneficial and logical.

11. Target Coordinates.

a. If scanning by geographical coordinates is to be used, the coordinates must be selected by an unimpeachable person not otherwise connected with the experiment or familiar with other persons related to the experiment. The coordinates should describe a variety of targets so that a Subject may not try to guess a particular type, some of which should describe innocuous sites. Preferably these should also vary in geographical location such that a Subject could not memorize detailed maps of any given geographical area. A possible approach is selection by random number of a large (greater than 5000) list of worldwide targets of interest.

b. The coordinates should be transmitted just prior to the viewing time. No maps and no feedback during the experiment should be allowed. Again, there appears to be no need for an experimenter to be present. The Subject should complete his/her viewing in a pre-set time period and only one viewing should be allowed. Details of the viewing should be relayed immediately via a secure computer network or other similar form of communication. Again, if feedback is used, only the Subject should be given feedback.

12. Reporting. A major problem with research in this field is the incomplete, inexact, erroneous, and duplicate reporting. All experimental details, responses, instructions, transcripts, etc., must be reported, however lengthy and laborious the task might be. Only in this manner will the "loyal opposition" be able to satisfy their desire for facts and re-analysis. Only then must they resort to a malfeasance or dishonesty criticism.

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13. Application to Intelligence Systems. Research conducted and reported to date has a number of inaccuracies, inconsistencies, and methodological weaknesses sufficient to cause concern over its validity. If all results are accepted without question, on balance the fidelity of the remote viewing channel appears to be of limited intelligence value. However, that conclusion may be totally premature due to the insufficient methodologies used. To assess validly the value of the remote viewing channel for operational use, much more careful research is required, preferably by several laboratories following the same (improved) protocols with detailed documentation. It would be particularly desirable to have different researchers (i.e., laboratories) conduct experiments with the same experienced Subjects (e.g., Swann or Hammid). In this manner, the reliability of the remote viewing channel can be assessed, with a "known capability" Subject, yet satisfy the demands of the "loyal opposition" by having replication of the research by an independent research team using the same protocol.

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